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ARE ACQUIRED VARIATIONS INHERITED?

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ARE acquired characteristics inherited? We admit that individuals inherit a certain constitution, and that definite variations from this constitution are acquired during life-time, according to well-known laws. The question is: Are these definite acquired variations in any degree transmitted, or are the congenital variations in the constitution of the offspring independent of those which have been acquired by the parents?

PRESENT STATE OF THE QUESTION.

Before opening this discussion let us draw up a balance sheet in biological philosophy for 1890, and determine exactly where we stand in point of knowledge of natural causation. Fortunately Professor Huxley balanced the Evolution account in 1871[1] in his usual accurate and candid manner, enabling us to institute a comparison:

"If I affirm that 'species have been evolved by variation[2] (a natural process, the laws of which are for the most part unknown), aided by the subordinate action of natural selection,' it seems to me that I enunciate a proposition which constitutes the very pith

[1] "Critiques and Addresses," p. 299. *Contemporary Review*, 1871. This passage is practically a résumé of the article entitled "Mr. Darwin's Critics," in which it occurs

[2] Including under this head hereditary transmission.

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and marrow of the first edition of the 'Origin of Species.' And what the evolutionist stands in need of just now is not an iteration of the fundamental principles of Darwinism, but some light upon the questions, What are the limits of variation? and, If a variety has arisen, can that variety be perpetuated, or even intensified, when selective conditions are indifferent, or perhaps unfavorable, to its existence?"[3]

Thus, twenty years ago, Huxley declared Evolution well established, with the Law of Natural Selection as one of its well-determined factors, while he found that we were merely upon the threshold of knowledge of the laws of Variation. Some sanguine biologists of to-day believe we have crossed this threshold in the patient researches of the two intervening decades; but others are represented by Professor Lankester, who has now taken the rank of leading English critic, and has recently summed up our knowledge in an article,[4] presumably written with the greatest care and deliberation, as follows:

"Their causes (*i. e.*, the causes of variations) are extremely difficult to trace in detail, but it appears that they are largely due to a "shaking up" of the living matter which constitutes the fertilized germ or embryo-cell, by the process of mixture in it of the substance of two cells—the germ-cell and the sperm-cell—derived from two different individuals. Other mechanical disturbances may assist in this production of congenital variation. Whatever its causes, Darwin showed that it is all-important. . . . . Hence there is no *necessity* for an assumption of the perpetuation of direct adaptations.[5] The selection of the fortuitously (fortuitously, that is to say, so far as the conditions of survival are concerned) produced varieties is sufficient, since it is ascertained that they will tend to transmit those characters with which they themselves were born, although it is *not* ascertained that they could transmit characters *acquired* on the way through life."

[3] He then observes that Mr. Darwin formerly inclined to answer these questions in the negative, but latterly in the affirmative.

[4] "The History and Scope of Zoology." Enc. Brit. Vol. XXIV. Also "Advancement of Science," pp. 372-73.

[5] *i. e.*, of acquired characters.

The emphasis here is upon the contrast between our knowledge of the *fact* of variation (op. cit. p. 373) and our indefinite knowledge of the *causes* of variation.[6] In other words, we have been accumulating facts, and our present induction from them is that the variations which have formed the main basis of evolution are fortuitous; there may be, indeed, definite causes, but the effects are largely indefinite. Now if all, or even the great majority, of naturalists were in agreement with Lankester, we might claim to have made a distinct advance since 1870, even in having reached such a negative conclusion—that is, on the principle that we progress when we recognize that no further progress is possible.

But fortunately, or otherwise, this is not the case, for in opposition to those who share Lankester's opinions are an equally large number who would balance the account differently, and claim that the distinctive feature of the past twenty years of study is that we have reached some of the fundamental principles of variation which Huxley presented as the goal of research.

But this difference in the accounts does not stop here. We biologists are obliged to frankly confess to our fellow-scientists in chemistry and physics, and to the world generally, that after studying Evolution for a century we are in a perfect chaos of opinion as to its factors. There is actually no consensus as to the powers of the natural selection principle, none as to the laws of inheritance, none as to the influences of environment! In the very heart of this disturbance is the problem we have come together to discuss. It is the medium which refracts our judgment upon every one of the factors of evolution. We may continue to accumulate facts, but no actual advance can be made in the study of natural causation until this problem is absolutely settled one way or the other. This being the case, Weismann has done a monumental service in forcing this question to an issue. It is true a very large number of naturalists consider the question no longer *sub judice;* but as half this number hold one opinion, and the other half an opinion directly opposed, we are forced to the criticism that neither side can at present offer such a clear and full

[6] I am very desirous to give a perfectly fair representation of the views of all authors here quoted, and am aware that a single passage is often misleading. On the present subject compare other recent essays and reviews of Prof. Lankester, principally those in *Nature*.

demonstration of how evolution works upon their basis as to be conclusive; nor will either side admit the value of the evidence furnished by the other. Contrast two of our most vigorous writers on this point :[7]

"This is all the more necessary, in that this author (Weismann) and his followers repudiate the evidence upon which the claim is made that acquired characters, taken in the widest Lamarckian sense, can be transmitted. During a period extending over fifteen years, the present writer has devoted himself to a study of the genesis of adaptations, and with the lapse of time the conviction has grown only the clearer that these authors are laboring under a delusion. The way in which they have placed themselves upon record shows that they have not reckoned with the consequences of their reckless speculations."

A few months later Lankester, echoing Weismann,[8] writes to *Nature* :[9]

"Naturalists are at pressnt interested in the attempt to decide whether Lamarck was justified in his statement that acquired characters are transmitted from the parents so changed to their offspring. Many of us hold that he was not; since, however plausible his laws above quoted may appear, it has not been possible to bring forward a single case in which the acquisition of a character as described by Lamarck and its subsequent transmission to offspring have been conclusively observed. We consider that, until such cases can be adduced, it is not legitimate to assume the truth of Lamarck's second law."

Nature of the Discussion.—Before taking up the question of evidence as to this factor in evolution, let us clearly understand what we are *not* discussing at the present time. First, the law of natural selection is well established and no longer under discussion; it furnishes by far the best, in fact the only, explanation which can be offered for many adaptations,—the question before us

[7] Ryder. "A Physiological Hypothesis of Heredity and Variation." AM. NATURALIST, Jan., 1890, p. 85.

[8] "There are no observations which prove the transmission of functional atrophy or hypertrophy, and it is hardly to be expected that we shall obtain such proofs in the future." Biol. Memoirs, 1889, p. 429.

[9] March 6th, 1890, p. 415.

is only as to the extent of its action. Second, we need not discuss the inheritance of mutilations, for mutilations are not part of the regular order of nature, and while they might have strong positive, they have little negative, value; the elaborate arguments which have been recently directed against them, remind us, therefore, of Don Quixote's excursions against the windmills, as if Lamarckism mainly depended upon such evidence. Nor is it in dispute whether the effects of general atrophy or hypertrophy of the body are transmitted, for it is self-evident that an ill-fed organism will not bear as perfect offspring as a well-fed organism. As to pathological atrophy or hypertrophy, it is, I believe, admitted on both sides that in cases where it arises from certain bacilli it is possible that it may be transmitted with the bacilli. What we *are* discussing is whether the special and local variations in function and structure induced by environment and habit in the life of the parent tend in any degree to reappear in the offspring.

This is the modern or modified form of Lamarck's law. His followers admit that he overestimated the rate of inheritance of the effects of use and disuse in stating that *all* that is acquired is transmitted.[10] The element of rate or time is a secondary one, as it is with the law of Selection; the main point is whether such effects are transmitted at all. Of course there are Lamarckians of all degrees of fervor. The following statement probably reflects the average opinion:

1. In the life of the individual, adaptation is increased by local and general metatrophic changes, of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the reactions are greatest. 2. The main trend of variation is determined not by the transmission of the full adaptive modifications themselves, as Lamarck supposed, but of the disposition to adaptive atrophy or hypertrophy at certain points.[11]

[10] "Quatrième loi: Tout ce qui a été acquis, tracé ou changé, dans l'organization des individus, pendent le cours de leur vie, est conservé par la génération et transmis aux nouveux individus qui proviennent de ceux qui ont éprouvé ces changements."

[11] Osborn. "The Paleontological Evidence for the Transmission of Acquired Characters." Brit. Assoc. Reports; Proc. Am. Assoc. Adv. Science; 1889.

Dr. W. H. Dall has given a very full and carefully considered statement in his paper on "Dynamic Influences in Evolution," May 8th, 1890.

At all events, this involves the Lamarckian principle, with all its necessary bearings upon our opinions as to Environment, Variation, Selection, and Inheritance. If we adopt it, we must accept its full consequences. Taking Spencer's definition of Life as the continuous adjustment of internal relations to external relations, we must regard the race as in part the summation of these individual adjustments, in part as the summation, by Selection, of favorable fortuitous variations. Environment must act directly in producing variations in the organism as a whole; directly also it must produce special variations wherever it induces changes of function. As these variations are in a degree transmitted, we will discover some of the laws of variation in the study of individual adaptation; variations of this kind will be found in definite lines; indefinite variations will also arise from the fortuitous combination of individual characters; the proximate causes of variation must be changing environment as well as the combination of diverse individual characters. Selection, so far as it is here involved, will be found to act mainly upon the *ensemble* of characters which have their origin in individual variation by the extinction of unadapted individuals and races, but its action upon fortuitous variations will be concomitant. Inheritance must bear the burden not only of ancestral and race characters, but must accumulate the modifications of these characters which occur in individuals.

Let us associate the opposite principle, that special individual variations are not transmitted, with the name of Weismann, for at a time when Lamarck's principle was rising in favor[12] he boldly opposed it *in toto*. His doctrine of the continuity of the germ-plasma, and especially of the isolation of the germ-cells from influences which are exerted upon the body-cells, is a perfect and necessary complement of the doctrine that Evolution has advanced by pure Natural Selection; he carries these twin doctrines out to their legitimate conclusions. Recalling Spencer's definition and applying Weismann's principle, we must regard the race not as the summation of individual adjustments, but as the summation of the best adjusted germ-plasmata. Environment may act

[12] In 1883, when Weismann published his first essay on Heredity, the only English or American naturalist of note who was not subscribing to some form of the Lamarckian principle was Alfred Wallace.

directly in causing the organism to vary as a whole, but none of the special individual variations which it also produces indirectly and directly can be inherited; its influences upon the germ-plasma are gradual and indefinite. The lines of variation are definite so far as they are limited by the specific nature of the organism; within these limits variations must be indefinite and numerous;[13] the proximate cause of variation is the combination of the diverse individual characters of the parents. Selection must accumulate minute existing variations in the required direction, and thus create new characters;[14] it must act upon minute variations in single characters, as well as upon the *ensemble* of characters. Inheritance is the unbroken transmission of race and ancestral characters by subdivision of the germ-plasma; only changes which affect the body as a whole can be added to the characteristics of the germ-plasma.

This is a mere abstract of the diverse positions upon every problem to which these principles of Lamarck and Weismann lead us. No half-way ground is tenable; the result of this inquiry will be a complete rout to one side or the other. By the former we diminish the powers of Natural Selection, and increase the powers of Environment; at the same time we greatly simplify the problem of Variation, and render far more complex the problem of Inheritance. By the latter we throw the entire burden of Evolution upon Natural Selection, and eliminate the direct action of Environment; we admit definite laws or causes of general Variability, but no definite laws governing the variations of single characters; we greatly simplify the problem of Inheritance. In short, the vulnerable point with the Lamarckians is in solving the problem of Heredity, while their opponents are weakest in solving the problem of variation. From the purely theoretical standpoint both sides can offer a good working explanation of the process of Evolution, provided we grant all their premises; our duty as professed scientific men should be, therefore, to dispassion-

[13] Biological Memoirs, p. 288. "It is the specific nature of an organism which causes it to respond to external influences along certain definite lines, although these may be very numerous."

[14] Biological Memoirs, p. 275.

ately examine how far these premises accord with *all* the phenomena which we can actually observe in Nature, and then espouse the side which is most favored by probabilities. Now I have no hesitation in saying that neither side is showing the disposition to test their premises by all the observed phenomena, and this is one of the most hopeless features of the present situation.

Variation, Repetition, Regression.—All the factors of Evolution interact. Variation and Repetition[15] in inheritance are in constant relation with every other factor. Thus we can accumulate facts as to variations *per se*, but if our observation and induction enable us to formulate certain laws, these will always involve at least two factors,—*i. e.*, Variation as related to Environment, Variation as related to the life-history of individual organisms, Variation as related to Inheritance, Variation as related to Natural Selection.

Variability is, of course, exhibited in organisms as a whole, and in groups of characters as well as in single characters. All would be diversely affected by the two diverse principles of inheritance under discussion, but we are to examine the variable tendency as exhibited in single characters. Repetition is the conservative or static condition wherein a character in the new individual most closely resembles the average development presented by the fraternity,[16] co-fraternity, race, variety, and species to which it belongs; let us adopt Galton's term "mediocrity" for this state of average development. Variation is the unstable or fluctuating condition in which a character deviates to either side of mediocrity, either in the plus or minus direction,—*i. e.*, to greater or less development. Regression is the tendency[17] to revert to "mediocrity"; and according to Galton's statistics we can imagine this law of regression as acting like gravitation upon the pendulum of variation: when the pendulum swings in one direction it may repre-

[15] Weismann, or his translators, uses the terms Variability and Heredity, as tendencies equivalent to these. But it seems to me clearer to use Heredity in the larger sense, so as to include Variation = the act of Varying, and Repetition = the act of repeating, ancestral characters. Variability = the tendency to vary.

[16] Galton. "Natural Inheritance," p. 94. All the offsrpring of the same mid-parne (= male and female) form a fraternity. All the offsping of a fraternity of mid-parentst form a co-fraternity.

[17] Op. cit., p. 95.

sent a plus-variation, in the other direction a minus-variation; mediocrity is the state of rest or balance. When we examine any species in course of evolution in time and space we find, however, that a mediocre character is a shifting quantity. An organ, for example, which is rapidly degenerating presents a certain "mediocrity" at one time and locality, and another "mediocrity" at a later time or another locality. There is, therefore, a clear distinction between the above terms and the more general terms "degeneration," "balance," and "development," which apply to characters which are either continuously static or in a downward or upward direction, not only in individuals but in whole species and larger divisions. Of course where regression ceases to exert its full gravitating force upon plus- or minus-variations, through a series of generations, development or degeneration respectively set in.

Another source of confusion, which is inevitable in observation but not in theory, is the difficulty of distinguishing between "congenital" and "individual variations." Weismann has marked the distinction by the useful terms "blastogenic" and "somatogenic."[18] Theoretically these congenital and acquired variations are quite distinct; but as some blastogenic variations do not manifest themselves until advanced life, it is extremely difficult in many cases to decide how far certain variations are really blastogenic and how far somatogenic in origin; in other words, how far they are due to inherited predispositions and how far due to life habits.[19]

A war of words has recently been waging as to the meaning to be attached to such adjectives as "fortuitous," "chance," "kaleidoscopic," or "indefinite." My understanding of these terms is that when we see characters fluctuating from mediocrity, in either the plus or minus direction, according to the ordinary laws of chance, we may describe them as in a state of indefinite variability; whereas, when they exhibit a tendency to fluctuate principally in one direction, we describe them as in a state of

[18] *i. e.*, as arising from the germ-plasma and body respectively.

[19] In his review of Wallace's "Darwinism" Lankester has pointed out this defect in some of Wallace's observations. *Nature*, 1889, p. 567.

definite variability. This is the only sense in which the terms "definite variations" and "indefinite variations" can be fairly used in this discussion.

In two of his most recent essays Weismann says:[20]

"My theory might be disproved in two ways,—either by actually proving that acquired characters are transmitted, or by showing that certain classes of phenomena admit of absolutely no explanation unless such characters can be transmitted. Only if it could be shown that we cannot now or ever dispense with the Lamarckian principle would we be justified in accepting it."[21]

We may gather evidence from the data of Embryogeny, or of Ontogeny and Phylogeny. It is neither possible nor desirable to separate these data; but as previous writers have dealt extensively upon the evidence of embryogeny, I will emphasize the ontological and paleontological evidence, with which I am, in fact, much more familiar. I shall endeavor principally to concentrate attention upon the phenomena to which future observation must be especially directed. We already have a number of valuable essays and criticisms in this line,[22] but none, so far as I have seen, examine the question in view of all the difficulties which the adoption of either principle involves us.

I believe we are far from understanding all the phenomena of variation, and put the question, therefore, in the following form: Does our present knowledge of variation in living and fossil forms lend greater support to Lamarck's or to Weismann's principle?

1. *What is the Origin of Variability?*—According to Weismann, the *ultimate* or primordial origin of variability is somatogenic,[23]—that is, we must trace variability back to the unicellular organisms in which the environment acts directly upon the whole

[20] Biol. Mem., p. 388.

[21] *Nature*, Feb. 6th, 1890, p. 322.

[22] Especially those of Ryder, Cope, Eimer, and Cunningham. A very valuable review of the whole subject is found in C. V. Riley's paper, "On the Causes of Variation in Organic Forms." Proc. Am. Assoc. Adv. Sci., 1888.

[23] Biol. Memoirs, p. 277. "The origin of hereditary individual variability cannot be found in the higher organisms, but it must be sought for in the lowest,—the unicellular organisms. . . . As such organisms reproduce by division, individual acquired characters will be transmitted to the offspring."

organism; in the multicellular organisms the source of variability becomes restricted to the germ-cells, and the proximate or secondary origin of variations is in the union of the diverse characteristics contained in the germ-plasms of the two sexes. This view as to the primordial origin of variations does not seem to me to enter directly into the problem we are discussing, although it is one of the legitimate conclusions from his premises. But I would like to call attention to one important point, viz., that it involves the operation of Lamarck's principle of the transmission of adaptive reactions to environment [24] in the unicellular, and therefore to some degree in the lower multicellular, organisms. I think it can be shown that Lamarck's principle would be highly advantageous to every organism by transmitting direct adaptations (see Query 4); if this be the case, every step in the gradual loss of this principle by the isolation of the germ-plasma would have been disadvantageous. Therefore, if Selection was constantly acting, as Weismann supposes, it would have preserved this very principle. This is, of course, in the nature of pure speculation; but turning this supposed enormous power of Selection to the service of Lamarckism, we can conceive how the extremely complex correlation between functional changes in the somatic and germ-cells, which is an essential part of the Lamarckian theory, may have had its beginnings in these transitional organisms.

The question of the *present* or proximate origin of variations does, however, bear directly upon these diverse principles:

(*a*) All observers must agree that sexual reproduction is one of the endless sources of indefinite variations.[25] Weismann's theory offers a beautiful idea of the *modus operandi*, and accords thoroughly with Galton's researches. Such variations originate in the germ-cells; there is no reason why we should trace them to the somatic cells.

[24] This is, of course, no new idea. It was most fully elaborated in Spencer's "Principles of Biology." The mode of transmission in the former is by simple cell division; the principle of the continuity of original and acquired characters is the same.

[25] Weismann's latest view is that sexual reproduction is the most important, but not the *only* factor which maintains Metaphyta and Metazoa in a state of variability. *Nature*, Feb. 6th, 1890, p. 322. (In answer to Prof. Vines.)

(*b*) Some plus- or minus-variations must also originate from the union of germ-cells. If the same character is strongly developed in both parents, it may appear still more strongly developed in the offspring; the same rule applies conversely to weakly developed characters. But this simply puts the question one stage back, for variations which are indifferently plus, minus, or mediocre are certainly not definite, although the union of two similar variations produces a definite result.

Before considering the possible origin of definite variations we must consider whether there are such variations.

2. *What Variations are Definite and What Indefinite?* [26]—This is really the most important and central question. Its solution has a vital bearing upon Weismann's principle as well as Lamarck's. Following Huxley,[27] Geddes [28] has most clearly stated these bearings:

"In the absence of any theory of definite and progressive change,[29] and in the presence of multitudinous variations under domestication and in nature which we can neither analyze, rationalize, nor hardly even classify, we are not only justified but logically compelled to regard variation as spontaneous or indefinite,—*i. e.*, practically indeterminate in direction, and 'therefore unimportant, except as the groundwork for Selection to act on.' Conversely, variation must be indefinite, else the paramount importance of natural selection must be proportionally impaired as this becomes definite. . . . It would exchange its former supremacy as the supposed determinant among the indefinite possibilities of structure and function for that of simply accelerating, retarding, or terminating the process of otherwise determined change."

We cannot emphasize too strongly these cardinal factors of indefinite Variation (so far as adaptation is concerned) and paramount Selection as two of the foundation stones of Weis-

[26] "Natural Selection trusts to the chapter of accidents in the matter of variation." Lankester.

[27] Article "Evolution," Enc. Brit., Vol. VIII.

[28] Article "Variation," Enc. Brit., Vol. XXIV.

[29] Such as has been postulated by Gray, Nägeli, and Mivart, or based upon the Lamarckian principle by Spencer, Cope, and others.

mann's theory of Evolution. This must be kept in mind in analyzing every argument advanced by his school. (The idea is that variations are definite only so far as they are limited by the specific nature of the organism, by special phenomena of nutrition, or in some cases by environment acting directly upon the germ-cells.[30] See Query 3. They are indefinite so far as they arise from the fortuitous union of diverse germ-plasmata.)[31]

I have made it clear in the introduction that this is no longer a matter of ignorance, as it was professedly with Darwin:

"I have hitherto sometimes spoken as if the variations, so common and multiform with organic beings under domestication, and in a lesser degree with those under nature, were due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation."[32]

I have already quoted Lankester upon this principle, and refer below to a passage in which he reiterates it and carefully defines the sense in which "indefinite" is employed by him.[33] Prof. Thiselton Dyer, a leading English botanist, has supported this position:[34]

"If with Prof. Lankester we say that the combinations are kaleidoscopic, I do not see that we go beyond the facts. . . . The area of fortuity is narrowed down to the variable constitution of the ovum. . . . And this is quite in accord with the remark of Weismann that variation is not something independent of, and in some way added to, the organism, but is a mere expression for the fluctuations in its type."

[30] See Biol. Mem., p. 410.

[31] See Biol. Mem., p. 275. "Natural Selection must be able to do infinitely more than this: it must be able to accumulate minute existing differences (arising by these fortuitous combinations) in the required direction."

[32] "Origin of Species," 6th edition, p. 106.

[33] The latest is in *Nature*, March 6th, 1890. "This disturbance of the parental body (I compared it to the shaking up of a kaleidoscope), and with it of the germs which it carries, resulting in "sporting" or "variation" in the offspring, is, it should hardly be needful to state, a totally different thing to the definite acquirement of a structural character by a parent, . . . and the transmission to offspring of that particular acquired structural character."

[34] See Osborn. "Paleontol. Evidence," etc.

One reason why I have endeavered to emphasize the unanimity of opinion upon this point among those who deny Lamarck's principle in this: If there are definite lines in blastogenic variation which cannot be explained by Selection, or by Environment acting upon germ-cells, we must find some other causes or laws governing them. Therefore the Lamarckians must first establish their claim that there are definite lines of variation; second, that these lines have not been directed by Selection (see Query 6). The opinions of Lamarckians on this point is that "there are variations which follow from their incipient stages a certain definite direction towards adaptation, independent of Selection in their origin." [34] This, it will be observed, does not exclude the existence of variations of the class accounted for by Weismann, but it constitutes substantially a distinct class of variations which Weismann, Lankester, and others do not account for, because, upon their hypothesis, we have no evidence that there is such a class." [35]

This opinion has frequently been asserted without adequate support from observation, otherwise we should not find such candid writers as those quoted above dismissing it so summarily. The fact is, it is very difficult, if not impossible, to prove that there are definite lines of variation (which cannot be explained by Selection) from the examination of zoological and botanical collections, for we are, from the nature of the material, principally examining variations by divergence in space. In such complete fossil series as are now available paleontologists enjoy the distinct advantage of following divergence both in space and time. They are thus in a better position to study lines of variation than ever before, because they are in at the birth, so to speak, of many useful and adaptive characters, and can follow the gradual rise from the minute infinitesimal stages to the advanced condition in which are constituted what we call specific and generic characters. Not only so, but it is possible to observe pedigrees, since the condition of surrounding parts prior to their appearance is known.

The history of the teeth of the Mammalia affords the most

[35] That is, no class of variations which conform to direct individual adaptations.

direct evidence, since these structures furnish not only the most interesting correlations and readjustments (quantitative variation), but also the successive addition of new elements (qualitative variation). I believe the unanimous opinion of all those who have examined such series is that such variations follow definite lines from their incipient stages. This is a positive form of evidence, unless the observers are at fault, but cannot be considered as proof if it can be shown that these infinitesimal stages arise indefinitely, for if the advanced condition is useful the incipient condition must possess some degree of utility, and would *ex hypothesi* be selected. This objection is met, however, by the additional fact that the first appearance of such structures is also not indefinite,—*i. e.*, at definite adaptive points. In other words, the birth is as definite as the growth.[36]

To sum up, the opinions of the two sides as to the nature of blastogenic variations are as follows:

Both will admit:

I. That there are general fortuitous variations, which may be best explained as due to the spontaneous variability of the germ-cells, especially seen in their union.

II. That there is also a class of variations, also springing from the germ-cells, which are in one sense definite,—*i. e.*, in certain directions,—but not necessarily adaptive.

One side denies, the other affirms:

III. That there is also a large class of blastogenic variations which follow definite lines of adaptation.

What are the relations of these three classes of variations to environment?

3. *What are the Direct and Indirect Relations between Environment and Variability?*—How far does environment affect the germ-cells directly, and how far through changes in the somatic cells? It is well known that a change of environment, especially to more favorable conditions, as in domestication, increases Varia-

[36] Some idea of the enormous mass of material available may be gained from the recent generalization that the teeth of all the Mammalia have sprung from a similar type and passed through similar stages. See the papers of Cope, Wortman, and the writer.

bility,[37]—*i. e.*, variations of Class I. In the analysis of such effects effects we should carefully examine:

(*a*) Whether this variability in all the characters of the organism is an effect of the action of Environment directly upon the germ-cells, through the general channels of increased or diminished nutrition; or, whether the environment produces a general disturbance of the functions of the organism, and this acquired disposition to altered functions is transmitted to the germ-cells.[38]

(*b*) Whether changed environment produces variability in any special characters or in all characters alike? Here again the question as to the mediate action of the somatic cells comes up, and is not only much more pertinent than in (*a*), but probably more capable of solution.

On these points Weismann holds that luxuriance of growth results from the better nutrition of the germ-cells during development,[39] while poverty of growth, or general degeneration, conversely results from deficient nutrition of the germ-cells, as in the case of Falkland ponies.[40] The effects of these influences he thinks may be more specialized; they may act only upon certain parts of the germ-plasma.[41] Weismann discusses such cases as follows (p. 433). Observe that the modifications referred to are not necessarily adaptive:

"The wild pansy does not change at once when planted in garden soil; at first it remains apparently unchanged, but sooner or later in the course of generations, variations, chiefly in the color and size of the flowers, begin to appear; these are propagated by

[37] This we can attribute to the greater molecular activity of the cells. Darwin believed (*a*) that exposure to new conditions must be long continued to set up any new variation. (*b*) Excess of food increases variability. (*c*) Changed conditions may affect the whole organism, or certain parts alone, or merely the reproductive system. (*d*) Indefinite variability is the commonest result of changed conditions.

[38] The point raised by Mivart (*Nature*, Nov, 14th, 1889, p. 41) is not fairly taken. Of course nutrition must pass through some *somatic* cells of the digestive system on its way to the germ-cells; this is a different matter from its first passing to the peripheral somatic cells in certain organs and then conveying their modifications to the germ-cells.

[39] Biol. Mem., p. 98.

[40] Op. cit., p. 99.

[41] Op. cit., p. 104. Or as discussed upon p. 408, in the criticism of Hoffman's experiments upon flowers.

seed, and are therefore the consequences of variations in the germ. The fact that such variations *never* occur in the first generation proves that they must be prepared for by a gradual transformation of the germ-plasm. . . It is therefore possible that the modifying effects of external influences upon the germ-plasm may be gradual, and may increase in the course of generations so that visible changes in the body (*soma*) are not reached until the effects have reached a certain intensity."

The best-attested instances of the action of Environment in producing special characters are those seen in its action upon the reproductive organs. A slight change of conditions sometimes produces sterility, as seen in the cases of "isolation" and "divergence" advanced by Gulick and Romanes. Here the best explanation seems to be that the environment has acted directly upon the germ-cells. This could only be proved, however, by experiments in artificial impregnation, for it is possible that the cause of sterility might lie in some of the somatic functions accessory to impregnation or intercourse. A second instance of this kind is the effect of nutrition in the determination of sex, as proved by the experiments of Yung and Giron,[42] and employed as one of the main principles in the two theories of Heredity advanced respectively by Ryder and Geddes.

It is not necessary to enumerate the many well-known cases of rapid response to new environment by modifications, which we must analyze somewhat differently. Among the best recorded are those of Saturnia (imported to Switzerland from Texas),[43] and Artemia.[44] Now it is a very important fact that the modifications observed in such cases are in the main adaptive,—that is, in course of a very few generations not only are the organisms thoroughly acclimated, but they develop substantially new adaptive characters.

We can readily understand how the germ-plasm might respond

[42] Yung raised the percentage of females among tadpoles by high nutrition from 56 per cent. to 92 per cent. Giron found that sheep, when well fed, bred 60 per cent. males; when poorly fed, only 40 per cent. See Geddes and Thompson, "Evolution of Sex," Chap. IV.

[43] Recorded by Wagner. See "Die Entstehung der Arten durch Räumliche Sonderung." Basel, 1889.

[44] Schmankewitsch.

directly to new environment by general variability, and even by such special variations as above cited by Weismann; but, keeping in mind the fortuitous principle, why do we also discover variations, not merely in size and efflorescence,[45] but in the nature of direct adaptations? This point has recently been raised by Mivart, with his usual acuteness in destructive criticism.

I do not consider that it has been demonstrated that Environment does act directly upon the germ-cells. In the case of animals we certainly cannot determine how far the nervous and other somatic cells are mediate, besides the somatic cells of the nutritive system. Yet in the acceleration of variability, and in the direct production of variations of Class II., we have examples of such rapid response to changed environment that the presumption is somewhat in favor of Weismann's view. In either case, such mediate action of certain somatic cells cannot be advanced in support of Lamarck's principle that the effects of environment on special groups of somatic cells make themselves felt in, or transmitted to the germ-cells in such a manner as to reappear in some degree in the same special groups of somatic cells in the new individual. Let us therefore concentrate our attention upon the evidence as to the possible modes of origin and transmission of variations in definite adaptive lines (Class III.). Three explanations are open to us: 1. That these adaptations have been selected from a number of variations of the fortuitous class; 2. That the germ-cells respond to environment by adaptive variations; 3. That the variations originate in adaptive reactions of the somatic cells, under environment, which have been transmitted to the germ-cells. Let us first consider the question of individual variations.

4. *Are Individual Variations Adaptive?*—I should hardly have thought it necessary to consider this question but for the fact that a recent writer, who claims the sanction of Mr. Romanes and Mr. Poulton, has advanced the proposition that the inheritance of

[45] Huxley has thus analyzed Environment on the pure Selection hypothesis: "Environment does not cause a variation in any particular direction, but favors and permits a tendency in that direction which already exists. . . Conditions are not actively productive, but passively permissive." "Critiques and Addresses," p. 309.

individual variations would be an actual evil.[46] This is tantamount to saying that adults are less adapted to their environment than young individuals, and that the most perfect individual adaptation will be secured by inertia. This would, as Mr. Ball maintains, be a severe blow to the Lamarckian principle, but it would be a still more severe blow to the Natural Selection principle, for, to give a single instance, it can be shown conclusively that the skeleton of the limbs of all the Mammalia has mainly been evolved upon the broad lines of use and disuse, and Selection would thus be eliminated entirely. To express this idea of the utility of the greater part of individual variation, Semper applies the term "adaptations," and his work[47] abundantly illustrates and demonstrates this law. It is based, of course, upon the general physiological principle that the tissues react and their structure diversifies proportionally with their functions.[48] Life is the continuous adjustment of internal relations to external relations, in which the general adaptation of the organism to its surroundings is, upon the whole, steadily increasing up to the period of general decline.

This principle of individual adaptation is strikingly illustrated in recent studies upon the feet of the Mammalia, in connection with instantaneous photographs of animal motion.[49] These studies show, for example, in the extremely complex readjustments of the carpal bones, necessitated by the simultaneous reduction of one of the bones of the fore-arm and of the lateral toes, that the very redistribution of the lines of pressure is constantly tending to perfect the adaptation by the natural reactions of growth in the bone tissue. Some of these adaptations are in the nature of plus- or minus-variations from the original constitu-

[46] W. P. Ball. "Are the Effects of Use and Disuse Inherited?" Nature Series, 1890, p. 128.

[47] "Animal Life," 1877.

[48] Lamarck is still ridiculed for his idea that the wants or desires of animals produce new parts, but the only ridiculous points are in some of his illustrations of this idea. Every vertebrate is literally made up of "wish-bones" in one sense, since all parts are developed by the voluntary efforts of the animal to obtain its food, etc.

[49] See the papers of Cope and Ryder, and the writer's "Evolution of the Ungulate Foot." Memoir upon the Uinta Mammalia.

tion of the limb; other elements remain *in statu quo*,[50] or in a state of balance where their adjustments are perfect.

There is also a large class of adaptive, characters, both in animals and plants, upon which the law of individual adaptive variation operates very obscurely if at all,—*e. g.*, protective coloration

How Far Does Race Variation Follow Individual Variation?—The study of individual variations led Spencer to the conclusion that all higher forms (of vertebrates) have arisen by the superposing of adaptations upon adaptations.[51] The students of vertebrate paleontology observe that race adaptations conform so closely to the laws of progressive individual variation that they are impelled to seek the explanation of the origin of various structures in the reactions occurring in individuals. Here are the definite lines of variations spoken of above.

But if they jump to the conclusion that individual variations are the *cause* of these race variations, may they not fall into the old fallacy of *post hoc ergo propter hoc?* For every genetic line will be found to exhibit variations in definite lines of adaptation and many of these lines of variation occur in characters in which no individual adaptation can be observed.[52] Now there is no theoretical difficulty in supposing that the three classes of variations have different modes of origin, but in order to demonstrate the probability of a causal relation between individual and race variations of Class III. it is further necessary to show: 1. That in this special class of characters, in which obvious mechanical or dynamical principles are operative, race variations invariably conform to individual variations; for if some of these characters do not conform, other principles must be in operation. That is, if we once invoke the Lamarckian principle, we must apply it consistently to every case. 2. That no definite lines of variation arise in characters of this class without the antecedent operation of these individual reactions. These first tests of invariable antecedence and consequence would lend a high degree of probability

[50] A beautiful example of the effects of use in producing joints in the tail fins of fishes has been given by Ryder. Proc. Am. Phil. Soc., Nov. 21, 1889.

[51] As quoted by Ryder from *British and Foreign Medico-Chirurgical Review*, Oct., 1858.

[52] Such as are seen in the adaptations of mimicry and protective coloration.

to the existence of causal relationship; this probability would be increased if it could be shown that no other explanation of this class of variations will stand the same test.

First, as to sequence. The overwhelming majority of variations as observed in the fossil series[53] occur along the lines of use and disuse. Weismann has urged that all variations in this class are substantially quantitative, that where an organ becomes stronger by exercise it must possess a certain degree of importance, and when this is the case it becomes subject to improvement by natural selection.[54] It follows from embryological development and the laws of growth by cell division that all new characters are in one sense quantitative, but in tooth evolution we have examples of the rise of structures which are qualitative,—*i. e.*, essentially new, and not simple modifications of preëxisting forms. I refer to the successive addition of new cusps. As already observed, there is absolutely no evidence for indefinite variation in these characters. The new cusps do not rise spontaneously at random points and then disappear, to be replaced by the gradual development of those which happen to rise at adaptive points.[55] One of the most surprising recent discoveries is that one after another these successive cusps are added to the simple conical crown at the point of maximum wear; that is, the most-worn points in an earlier series of generations are those at which the new cusps appear in the later series.

Paleontologists cannot, however, claim that this sequence is universal. Among the rare exceptions there are, first, some secondary cusps[56] which arise from the base of the crown,—*i. e.*, entirely out of the region of use and disuse and pursue the same steady development until they reach a stage in which they are obviously useful and subserve attrition. Second, upon the principle that the action and reaction of two opposing surfaces must be equal, it is difficult to explain some cases in which we observe a

[53] See the exact studies of Kowalevsky, Cope, and Ryder among the vertebrates, and of Hyatt, Dall, and others among the invertebrates.

[54] Biol. Memoirs, p. 84.

[55] See "The Evolution of Mammalian Molars to and from the Tritubercular Type," AMERICAN NATURALIST, December, 1888.

[56] Such as appear in some molars of the later Tertiary ungulates.

cusp in one jaw developing, while the cusp in the other jaw, opposing it and presumably stimulating its development, is degenerating.[57] The force of these exceptions will weigh seriously against the Lamarckian principle, unless they also can be proved by subsequent research to conform to the laws of individual adaptation. I consider that the strongest line of attack which can in futute be taken against Lamarckism will be in showing that certain characters (such as the above), in which it is supposed to operate, could not be produced on principles of direct adaptation.

But if we reject the Lamarckian principles we must assign Selection as the cause of these definite lines in variation, for no one would urge the third alternative.

6. What is the Relation between Variation and Selection?—The question of Utility is the first which arises when we attempt to explain the origin of such variations as we are here considering by the selection principle. In the recent animated discussion which has taken place between Romanes,[58] Mivart, and others on the one side, and Wallace[59] and Dyer on the other, great difference of opinion has been shown as to Utility. So far as the question bears upon the substitution of pure natural selection for Lamarck's principle, we may, in this argument, avoid the broader question by admitting that all characters possess, or have once possessed, some degree of utility, or the reverse. This is as necessary for Lamarck's as for Weismann's principle. The essential question here is whether the plus- or minus-variations in advanced stages, or the variations in initial stages, or still more the variations which constitute the initial stages themselves, are of such importance as to weigh sufficiently in the scale of survival, to accumulate definite lines of adaptive variations.[60] Let us assume that they can be, what further assumptions are necessary?

We start with the proposition that all these variations have their origin under the laws which we have seen govern variations of Classes I. and II., for upon Weismann's principle we cannot admit

[57] I refer to the paraconid and hypocone.

[58] "The majority of specific differences are inutile (non-adaptive)." *Nature*, '89, p. 8.

[59] "There is no proof that specific characters are frequently useless."

[60] Darwin distinctly abandoned the utility principle in the case of Saturnia. See letter to Moritz Wagner, "Life and Letters," Vol. III.

any other modes of origin. They must start, therefore, indefinitely, but secure a definite direction by the selection of those in favorable, and elimination of those in unfavorable, directions. This direction must be continuously plus where the characters are developed by direct Selection,[61] or neutralized where the characters are under the sustaining power of Selection, or minus where the characters are degenerating under the influence of Panmixia (free intercrossing), or even of reversed Selection.[62] Every union of new individuals, according to Galton's law of regression, however, will tend to draw back all the plus- and minus-variations to mediocrity, even where both parents show a tendency in the same direction. This regressive tendency to mediocrity, seen in the union of a single pair, will be further hastened by Panmixia.[63] We have assumed the continuous operation of Selection and abundant favorable variations to draw from, but we have seen, under Query 3, that variability is generally greatest when external conditions are most favorable; at the same time Selection must be least active, for the struggle for existence is least severe,—that is, Selection is least rapid when its materials are most abundant. So much for the probabilities of the production of definite lines of variation in single characters of this class. Evolution is not, however, a "log-rolling" process, in which some parts lag behind while others are improved by selection; in the fossil series, as all parts of the skeletal organism are observed in course of evolution at the same time, we must assume indefinite variability in every part, and admit the probability that, especially in uncorrelated parts, the sum of favorable variations, will be equal to the sum of unfavorable variations, and thus neutralize each other, so far as Selection is concerned. We must, therefore, add the assumption that these definite lines will be selected in correlation with those observed to occur in all the surrounding parts, and granting that groups of correlated parts may

[61] See Biol. Memoirs, pp. 264, 85, 101, 275.

[62] Romanes has endeavored to show that where a character becomes detrimental Selection will tend actively to eliminate it.

[63] Galton has shown that in the union of two individuals showing exceptional characteristics only a few of the offspring would be likely to differ from mediocrity so widely as the mid-parent. "Natural Inheritance," p. 106.

vary simultaneously[64] (*e. g.*, fore and hind limbs, or a series of vertebræ), we have still further to assume that these variations are selected with coördinate variations in parts which are not in the remotest degree correlated, viz., the teeth.[65] We must still further assume that Selection acts at the same rate to produce simultaneously exactly parallel lines of adaptive variations in related species over widely distributed areas, as in the American and European species leading to the horse series. If it is maintained that this parallelism has been sustained by interbreeding, then the arguments based upon Divergence and Isolation lose their force. If it be said that combinations of favorable variations occur in nature, not only in correlated but in uncorrelated parts, and Selection acts upon these combinations, then those who support Weismann's principle must further assume that there are definite lines of blastogenic variation. This *argumentum in circulo* would bring us back to the original question, What is the cause of definite lines of variation?

Can Acquired Variations be Inherited?—It must be admitted by every one that, as the germ-cells are usually differentiated and set apart from the somatic cells at an early age, it is very difficult to conceive how definite changes in certain peripheral somatic cells occurring in the higher adult Metazoa can produce such changes in the germ-cells as to be reproduced in the offspring, even if we allow a very long time for the process. If, however, such a process does take place, it rests with the embryologists to work out a theory for it, so we are not concerned with the process, but the evidence. All the evidence above considered belongs properly to Evolution; we must now consider the bearings of some of the classes of evidence from Inheritance.

The evidence from mutilations is somewhat conflicting. It has been fully discussed recently by Weismann, Eimer, and others. It involves two elements which are not observed in the ordinary course of evolution: 1. Immediate transmission of the full characters. 2. Transmission of characters impressed upon the organism,

[64] The fact that they do so may be used as an indirect argument for the Lamarckian principle.

[65] Or adaptive characters for protection, mimicry, sexual ornamentation, etc.

and not self-acquired. I believe that no indisputable evidence for the inheritance of acquired characters has been produced under this head.

Another class of evidence consists in what are believed to be cases of the inheritance of maternal influences upon offspring *in utero*. It is an axiom among breeders that an ill-bred sire may affect all future strains. One of the most striking cases is that of Lord Morton's Arabian mare, which was sired by a Quagga, and later by a pure Arab, the foal of the latter showing zebra-like markings. Professor Turner says of this case: "I believe that the mother had acquired during her long gestation with the hybrid the power of transmitting quagga-like markings. The ova must have been modified while still in the ovary." [66]

I refer to papers of Vines [67] and Turner [68] as bearing especially upon the supposed isolation of the germ-cells, and showing that in the lower Metazoa and some of the higher Metaphyta the germ-plasm is diffused through the organism, and thus related to the *soma*.

We should find in these transitional organisms, as I have suggested under Query 1, that the relation between the somatic and germ-cells was established, if it exists. It is a necessary deduction from Weismann's theory that if this relation was advantageous it must have been preserved by Selection. If Selection can bear the burden of Evolution, it certainly can account for the origin of the Lamarckian principle in inheritance.

Conclusions.—The conclusions we reach in this discussion must finally turn upon the existence of definite lines of blastogenic variation. If there are no such lines, the Lamarckian principle falls *ipso facto;* if there are, we have still to estimate the probabilities between Weismann's and Lamarck's principles as affording the most adequate explanation for them, keeping in mind the problem of Inheritance as affecting these probabilities.

The Weismann principle depends upon Selection as the source

[66] See *Nature*, 1889, p. 532.

[67] "An Examination of Some Points in Prof. Weismann's Theory of Heredity." *Nature*, October 24th, 1889, p. 62.

[68] "The Cell Theory, Past and Present." *Nature*, November 6th and 13th, 1890.

of definite lines of Variation. What evidence has been advanced for the initial but all essential assumption that, for example, a tiny adaptive cusp is a factor in survival, while its tiny inadaptive fellow is a factor in extinction? not to mention the succeeding assumptions which overwhelm us when we seek to derive definite adaptation from indefinite variations.

The Lamarckian principle furnishes us with an explanation of the observed phenomena of simultaneous progressive adaptation in most of those parts which it affects, including Correlation and Parallelism. It cannot be said at present to explain *all* the phenomena within its sphere; we must explain these phenomena, or abandon the principle.

It follows as an unprejudiced conclusion from our present evidence that upon Weismann's principle we can explain Inheritance, but not Evolution, while with Lamarck's principle and Darwin's Selection principle we can explain Evolution, but not, at present, Inheritance. Disprove Lamarck's principle, and we must assume that there is some third factor in Evolution of which we are now ignorant.

ON THE ORIGIN OF THE GALAPAGOS ISLANDS.

BY G. BAUR.

ALL islands can be divided into two principal groups. 1. Islands developed from continents or larger bodies of land through isolation or subsidence: *continental islands.* 2. Islands not developed from continents, but from submarine portions of the earth: *oceanic islands.* The flora and fauna of the first group will be more or less *harmonic*,—that is to say, the islands will be like satellites of the continent from which they developed, and the whole group comparable to a planetary system. The flora and fauna of the second group will be *disharmonic*,—that is to say, it will be composed of a mixture of forms which have been introduced accidentally from other places. It is evident that the first group of islands will be affected in the same way; there will be immigrants from other localities besides the original inhabitants.

Continental islands, therefore, may be composed of two floral and faunal elements: first, an original (endogenous) one; and second a secondary (exogenous) one. Oceanic islands, however, will only contain a secondary (or exogenous) floral and faunal element.

We will now proceed to examine whether the Galapagos belong to the first or second group of islands; whether they are detached portions of a continent, or of new origin emerged from the sea. These islands, better than any other group, afford a splendid opportunity for the examination of this question. They had never been inhabited before their discovery by the Spaniards in the sixteenth century. Buccaneers and whalers made the islands a place of frequent resort; but it was not before 1832 that a small colony was established on Charles Island, which, however, was soon abandoned again. To-day a small settlement is found on Chatham Island only, under the management of Señor Manuel Cobos. The only animals which seem to have been seriously affected by these intruders are the tortoises, which had once been so very numerous on all the islands. On some of them at least—Charles Island, for instance—they are extirpated; but it is

probable that a few specimens still exist on some other islands on which they are now considered to be exterminated. All the islands, with the probable exception of Charles and Chatham, show with little doubt nearly the original condition.

The Galapagos are situated on the equator, about 600 miles west from the coast of Ecuador, to which state they politically belong. They are placed between the 89th and 92d degree west of Greenwich, covering about three degrees of longitude, and extending on each side of the equator one-and-a-half degrees north and south. (The best map published is No. 1375 of the British Admiralty, based principally on the researches of Captain Fitzroy, of the historical "Beagle.") There are five principal islands, eleven smaller ones, and a great number of islets and rocks. The five principal ones are situated between the equator and the 1° south. They are Narborough, Albemarle, James, Indefatigable, Chatham. Of the eleven smaller islands three are in the same region, Jervis, Duncan, Barrington; three are between the 1° and 2° south, Brattle, Charles, Hood; and five north between the equator and the 2° north, Tower, Bindloe, Abingdon, Wenman, Culpepper. Hood is the most southern, 1° 27′ south; Culpepper the most northern and also the most western island, 1° 39′ north, 92° west; Chatham is the most eastern, 89° 17′ west. Albemarle is by far the largest; it is the only one cut by the equator, and is about 140 km. long. Then follow Indefatigable, Narborough, James, Chatham, Charles, Bindloe, Abingdon, Barrington, Tower, Duncan, Jervis, Brattle; the last only about 1½ km. long and 1 km. broad. As is well known, the whole group is volcanic. The highest mountain, on the southwest end of Albemarle, is 1570 m. high. The elevation of the other islands varies from 70 m. (Tower) to 1270 m. (Narborough). Albemarle, and especially Narborough, the most western island of the larger ones, have frequently been in a state of eruption. Delano speaks of one on Albemarle in 1797 and 1800, Porter in 1813, and Morrell witnessed a most terrific outbreak on Narborough in 1825. Fitzroy saw smoke on a southeastern volcano on Albemarle in 1835. Since that time these volcanoes seem to have been in a state of inactivity. Wolf, who visited the islands in

1875, states that Narborough still has an active central crater of enormous size. Most of the islands consist of basaltic rocks and masses of scoriæ and lava, but besides we find, according to Wolf, remains of an older volcanic formation on Charles Island and the small islands Gardner, Caldwell, and Enderby, which surround it. Dr. Th. Wolf, the state geologist of Ecuador, visited the islands twice, according to a letter received from him. He spent in all six months on the group,—more time than any other visitor, Dr. Habel excepted. He published a small pamphlet about the islands in 1879 (Ein Besuch der Galapagos-Inseln. Heidelberg, 1879, 44 pp., 3 maps). The following account about the climate is taken principally from him and from "The South American Pilot."

The climate, considering that these islands are directly on the equator, is far from being excessively hot, a circumstance which is chiefly owing to the singularly low temperature of the surrounding sea, which is 23° C. On Charles Island at an elevation of 140 m. the thermometer was 19–21° (about 68° F.), and at about 300 m. 18–19°. Darwin observed a heat of 34° C. (93° F.) in his tent when the thermometer stood at 29° C. in the wind and sun, but which, when plunged in the soil, rose at once to 58° C., and would probably have risen higher had the tube been longer.

The rainy season is between February and June, but is very irregular, generally very short, and often it stops entirely for one or two years. In the higher portions of the islands, about 270 m., there is often rain all the year, but very little. In August and October Wolf observed such squalls, so-called "garruas," very often. They repeated each other often five or six times in a day, but never lasted more than half an hour. They are confined to the high plateau. The whole broad lower zone up to 170 m. is nearly without any rain; therefore the upper region remains always green, the lower one arid and barren. On the southeastern parts of the islands, on the side of the southern trade-winds, the damp region extends nearly 70 m. more down than on the northern side. It is evident that the conditions of the vegetation are dependent upon this different distribution of humidity. Near the border of the sea we find different maritime plants; in some of the bays mangroves

and avicenias. In ascending the hills from the shore the whole ground in all directions is covered with apparently withered brushes; but on a closer examination it is found that these plants are mostly in bloom. The most common brush in this region is a Lantana, one of the Verbenaceæ, and members of the Euphorbiaceæ, Croton for instance. This brushwood grows up to a height of 5 feet or 6 feet, rarely 10 feet, and here and there are found Algoraba trees about 20 feet high, and also sporadic Palo Santos (Guaiacum). The latter is the largest tree in the lower region; it reaches a height of 30 feet, and 3 feet in circumference. On places which do not allow the growth of any other plant, the grotesque, tree-like Opuntias and gigantic Cereus are found.

The Cereus is generally seen in the most barren spots. These forms give a very characteristic appearance to this region. The Opuntia reaches a height of 20 feet, and a thickness of two feet; the Cereus the same proportions. Besides these characteristic plants are found Gramineæ, Cyperaceæ, Euphorbiaceæ, Labiatæ, Compositæ, and so on (about fifty to sixty species according to Wolf). There are only a few Cryptogamia, the most important of which is the Orchilla (*Rocella tinctoria*), which is found not higher than 100 m. This is the condition of the vegetation up to 200 m. Now other shrubbery, especially Compositæ, appears; the Algoraba and Palo Santo are of stronger growth; the Lantanas and Cactaceæ disappear. The ground is still more covered with brushwood of the withered aspect. A white Usnea, which hangs in long strings from the trees, alone indicates a little more humidity. This plant is characteristic of a zone between 200 m. to 270 m., separating the dry and humid region. It is easily distinguished from a far distance by its white color.

When the high plateau between 270 m. to 300 m. is reached, the whole scenery changes. A refreshing, moist breeze comes from the coast; the traveler is surrounded by green woods and stands on meadows. These woods are principally composed of about thirty-feet-high trees, of the Sanguisorbaceæ and Compositæ, of an Andean type. Herr Wolf remarks: Everybody who knows the flora of Ecuador must be reminded of the mountains of the

Andes, and he appears to be on an elevation of about 3,000 m.; but in fact he is only 300 m. high. Wolf found great resemblance with the small Paramo forests of the Andes, not only in the habitus of the trees, but also in the small plants which cover the ground and in the mosses and lichens which cover the trees. The woods are free, without creeping plants, making a passage easy. They are interrupted by small meadows, consisting nearly entirely of small Gramineæ and Cyperaceæ. Above this woody region another one could be distinguished, which is free from trees, and only covered with short grass.

The description of these different zones is based on the condition found on Charles Island; but it is the same, according to Wolf, on the others of high elevation. From this it is evident that different islands which do not reach to the humid region, like Hood, Barrington, Tower, and so on, show only the arid state.

After this description of the climatological condition of the islands, I have to make some remarks on the topography of the whole group. The deepest sounding on record is 671 fathoms, between Tower and Indefatigable Islands; between the median islands the greatest depth does not surpass 300 fathoms; but a complete series of soundings between the different islands may bring to light quite different results. There is little doubt, however, that all the islands, Culpepper and Wenman perhaps excepted, are placed within the 1000-fathoms line. Northeast of Chatham, 0° 24′ south, 80° 6′ west, 812 fathoms are recorded by the "Albatross." It is probable that an elevation of 300 fathoms or 550 m. would bring all the central islands together.[1]

I give now the approximate distances between some of the islands: Hood to Culpepper, 430 km.; Chatham to Narborough, 262 km.; Hood to Chatham, 50 km.; Hood to Charles, 64 km.; Chatham to Indefatigable, 65 km.; Albemarle to Abingdon, 77 km.; Abingdon to Bindloe, 23 km.; Bindloe to Tower, 50 km.; Abingdon to Wenman, 141 km.; Wenman to Culpepper,

[1] The soundings of the "Albatross" have shown that the Galapagos are connected through Cocos and Malpelo Islands with Central America by the 4000-m. line. This is an important fact; all the older maps showed the Galapagos separated from Central America.

36 km.; Duncan to Indefatigable, 10 km.; Jervis to James, 8 km.; Barrington to Indefatigable, 18 km.; Indefatigable to James, 19 km.; James to Albemarle, 18 km.; Charles to Indefatigable, 50 km.

Currents.—From the "South American Pilot" I record: "The currents about these islands are remarkable, for, in addition to their velocity, which is from one to two-and-a-half miles an hour, and usually towards the west and northwest, there is a surprising difference in temperature of the bodies of water moving within a few miles from each other. These striking differences are owing to the cool current coming from the southward along the coasts of Chili and Peru, which at the Galapagos encounters a far warmer body of water coming from the Bay of Panama. Heavy rollers occasionally break upon the northern shores during the rainy season, though no wind of any consequence accompanies them. They are probably caused by the northers from Tehuantepec, and the Papagayos or northeast winds, which are so well known on the coast between Panama and Acapulco." In the vicinity of the islands calms prevail from January to April; from the middle of April to the end of December the trade-wind blows with regularity, and gales are unknown.

After these necessary remarks we can proceed to examine the fauna and flora.

1. *The Fauna.*—I shall restrict myself to the higher vertebrates, of which I shall give what is known. The only mammals which have been recorded are a mouse, two species of seals, and bats. The mouse is of the American genus Hesperomys. Darwin found it on Chatham, and Wolf saw a skin of it on Barrington Island. Further researches must decide whether this form is introduced or not. The fact that Wolf found it on the deserted Barrington speaks for its being indigenous. The seals found on the islands are *Otaria jubata* and *Arctocephalus australis*. They are still numerous, but in former times must have been exceedingly abundant, as I conclude from the work of Morrell. These two seals belong to Antarctic forms, and show in the Galapagos their most northern extension. It is quite probable that they represent species distinct from the more southern forms. So far as I know,

only the skeletons have been studied (Allen, "Pinnipedia"). Bats have been seen by Habel, and are also mentioned by Wolf; but no specimens have been collected so far.

Before speaking about the birds, I shall treat the reptiles, which are represented by tortoises, lizards, and snakes. The gigantic land tortoises always have attracted attention. The Spaniards gave the name Galapagos to these islands, which means tortoise. I may mention here that the etymology of the word Galapago is not known; the regular Spanish word for tortoise is tortuga, as in the Portuguese. The Portuguese also have the word cacado, which is of South American origin. It is probable that the word Galapago is of Central or South American origin also. I have treated on the tortoises in a paper published lately in the AMERICAN NATURALIST, and I shall give my results here in brief.

It was David Porter, the well-known American commodore of the "Essex," who pointed out for the first time the important fact that the different islands contain different races of the tortoise. Darwin has fully supported this view. He states that the inhabitants of Charles Island could tell from the aspect of the tortoise from which particular island it came. Tortoises have been recorded from Hood, Charles, Chatham, Indefatigable, Duncan, James, Albemarle, Abingdon. Nothing is known in this regard about Barrington, Brattle, Jervis, Bindloe, Tower, and the others. So far we know, there are six races of land tortoises from these islands: 1. *T. elephantopus* Harlan = *T. vicina*, Guenther; from South Albemarle. 2. *T. galapagoënsis* Baur = *T. elephantopus* Jackson; from Charles Island. The only complete specimen is in the Museum of the Boston Society of Natural History. 3. *T. abingdonii* Guenther = *T. cphippium* Guenther; Abingdon Island. 4. *T. microphyes* Guenth.; North Albemarle. 5. *T. guentheri* Baur = *T. elephantopus* Guenther; locality unknown. 6. *T. nigrita* Dum. and Bib.; locality unknown.

It is impossible yet to determine to which island the two last tortoises belong; but the important fact remains that the tortoises are different on the different islands. The species from Abingdon Island, for instance, is not found on any other one; the Charles Island form is only found there, and so on.

The genus Tropidurus of the Iguanidæ shows better than any other form the peculiar specialization on the different islands. I have already in a former paper spoken about the variation of this lizard. I reached the following result:

1. *Each island has only a single variety or species of Tropidurus.*
2. *Nearly every island has a peculiar variety or species of Tropidurus.*

These results were based on 128 specimens of Tropidurus collected by the "Albatross" on eight islands, namely: Chatham, Hood, Gardner (a little islet northeast of Hood), North Albemarle (Tagus Cove), James, Duncan, Indefatigable, Abingdon.

The first species of Tropidurus described from the Galapagos was *T. grayii* Bell, the type specimens of which had been collected by Darwin on Charles and Chatham Islands. I have taken the form from Charles Island, of which the "Albatross" did not get any specimens as the type of *Tropidurus grayii*, because Chatham Island is inhabited by a distinct form, which has been described as *T. lemniscatus* by Prof. Cope. Specimens from Indefatigable and James Islands are very much alike, and probably different from the specimens from Charles Island. I named these *T. indefatigabilis*. The peculiar form from Duncan Island was called *T. duncanensis;* that from Hood and Gardner Islands *T. delanonis;* the Albemarle form *T. albemarlensis;* that from Abingdon *T. abingdonii;* the Bindloe forms were considered as typical for *T. pacificus* Steind. No conclusion could be reached in regard to *T. bivittatus* Peters and *T. pacificus* (var. *habeli*) Steind., the localities of these forms not being known.

The following table shows the number of scales round the middle of the body in the different forms: *Tropidurus indefatigabilis* Baur, 55–59; *Tropidurus lemniscatus* Cope, 55–61; *Tropidurus albemarlensis* Baur, 57–63; *Tropidurus grayii* Bell, 55-65; *Tropidurus duncanensis* Baur, 72–79; *Tropidurus delanonis* Baur, 82-85; *Tropidurus pacificus* Steind., 85-90; *Tropidurus abingdonii* Baur, 95–101.

The specimens from Jervis figured by Steindachner as *T. grayii* are probably identical or closely related to *T. indefatigabilis*. Nothing is known of these lizards from Brattle, Barrington, Tower, Wenman, Culpepper, Narborough. I do not doubt that on most

of these islands we will find other species of Tropidurus. It would be of special interest to know whether any of them are found on Narborough, since we have to suppose that probably every organism was killed by that great eruption mentioned by Morrell. No other animal or plant shows the specialization of a single genus in the different islands so well as Tropidurus. On every or nearly every island it has developed into a different race or species. Let us now, for instance, compare the forms from Gardner and Duncan Islands with those from the nearest islands. On Gardner Island we find the same species as on Hood Island. Gardner is only one kilometer distant from Hood. The water between the two is not deep, not exceeding five fathoms. Between these islands are placed four smaller islets and the Magicienne Rock (Brit. Adm. Chart, 1376).

The species from Duncan Island is totally different from that of Indefatigable. The islands are only ten km. distant from each other. The deepest sounding taken between the two is, however, 60 fathoms. No specimen of *T. duncanensis* occurs on the very near Indefatigable, and none of *T. indefatigabilis* on Duncan. The races from the distant Abingdon and Bindloe and the low Hood Islands are more different from the central forms than those among each other.

Besides the genus Tropidurus, which is only found in South America, we have two other genera of Iguanidæ which are peculiar to the islands: the land and sea guanas, as called since the times of Dampier: Conolophus and Amblyrhynchus, the latter reaching a length of nearly four feet.

Amblyrhynchus is the only "oceanic" form of the Iguanidæ. It lives upon sea-weeds, and to get them it goes out to the sea, but never far from the shore. It is probably found on all islands. The "Albatross" collected specimens on Abingdon, Duncan, Hood, Gardner, James, Chatham, and it has been recorded from other islands, Charles, Barrington, Jervis, Albemarle, by Darwin, Steindachner, Wolf, and others. The individuals of the different islands have never been studied carefully; but I do not doubt that even here we find modifications according to islands. Perhaps the thirteen specimens brought back by the "Albatross" could

give some light when closely examined. Darwin already mentions differences. According to Steindachner, only small specimens are found on Jervis Island. Wolf found the specimens from Barrington different from those from Charles; they were all smaller and the dorsal spines were not so much developed. There is, therefore, evidence that even these semi-aquatic animals show some modification on the different islands.

Conolophus.—This large land form, no specimens of which were collected by the "Albatross," is so far only recorded from Albemarle, Indefatigable, Barrington, and James. Porter already states that it is absent from Charles. Nothing is known about the variation of this lizard, few specimens only having been collected. The Geckonidæ are represented by the genus Phyllodactylus. Charles Island contains a peculiar form, *Ph. galapagoënsis* Peters, of which the "Albatross" got one specimen. Two other species are recorded by Prof. Cope from Chatham, *Ph. tuberculosus* Wiegm., and *Phyllodactylus leei* Cope. *Ph. tuberculosus* would represent the only lizard not peculiar to the group, and Chatham would be the only island containing two species of the same genus. These specimens need reëxamination.

Snakes.—Snakes are much more widely spread over the islands than is generally believed. They were first mentioned by Dampier. Delano found snakes on Hood; Porter saw "a few small snakes much resembling the common American striped snake" on James, and "a small gray snake" on Albemarle; Steindachner, who distinguishes two varieties, *Dromicus chamissonis dorsalis*, and *D. habeli* records snakes from Indefatigable, Hood, Charles, and Jervis. The "Albatross" collected two snakes, one on James and one on Gardner Island. According to Dr. Habel, snakes are absent from Bindloe and Abingdon. We have, therefore, not less than seven islands on which snakes have been seen. How far the variation in the different islands goes is not yet known.

The Birds.—With the birds we are somewhat better acquainted, due to the collections of Dr. Habel and the "Albatross." The first collection forms the material of Mr. Salvin's extensive memoir on the Avifauna of the Galapagos. The material brought

together by the "Albatross" has been very thoroughly worked up by Mr. Robert Ridgway. Collections have been made by Darwin, Kinberg, Dr. Habel, and the "Albatross." A few specimens have also been collected by Dr. W. H. Jones, Kellett and Wood, Dr. Neboux, Cookson and Markham. I give a list of islands with the names of the collectors:

Chatham: Darwin, Kinberg, Jones, Kellett and Wood, "Albatross." Charles: Darwin, Neboux, Kinberg, Jones, Cookson, Markham, "Albatross." Indefatigable: Darwin, Kinberg, Habel. "Albatross." James: Darwin, Kinberg, "Albatross." Bindloe: Habel. Albemarle: Darwin, Cookson, "Albatross." Abingdon: Habel, "Albatross." Duncan: "Albatross." Hood: "Albatross."

We have, therefore, collections from nine islands. So far 69 species of birds have been described, besides the albatross, which is recorded by Delano and Wolf from Hood Island, Thirty-two are found on Indefatigable, 28 on Chatham, 27 on James, 23 on Charles, 18 on Abingdon, 14 on Bindloe, 12 on Hood, 5 on Duncan, and 4 on Albemarle. The great number of Indefatigable is due to the collection of Dr. Habel, who brought not less than 267 skins from this island. Albemarle has hardly been touched. Darwin collected one bird. The "Albatross" remained only five hours at Tagus Cove, and collected four species. There can be no doubt whatever that a very much greater number of birds exist on Albemarle. We know from Wolf that some parts of Albemarle, especially the southern ones, have a well-developed flora (fifty to sixty species of Phanerogams in the lower regions) and there must be also birds.

No collections have been made on Brattle, Barrington, Narborough, Jervis, Tower, Wenman, and Culpepper. The number of genera collected is 40; of these 6 are peculiar to the islands, 8 are found in continental America, and 27 have a wide distribution. Of the 69 species 49 are peculiar to the islands, or 71 per cent. The six peculiar genera are Nesomimus, Certhidea, Geospiza, Cactornis, Camarhynchus, and Creagrus. Nesomimus belongs to the Mimidae; Certhidea to the Cœrebidæ or Honey Creepers; Creagrus to the Laridæ; and the three others to the Fringillidæ or finches.

Creagrus contains a single species, and all the specimens the history of which is absolutely known were taken on Dalrymple Rock, near Chatham Island, a rock 65 feet high,—a most peculiar example of distribution of a single genus.

The genus Nesomimus is represented by five species; one is peculiar to Charles, one to Hood, one to Albemarle, and one to Abingdon; the fifth, *N. melanotis*, is found on the central islands, Charles, Chatham, James, Indefatigable.

Certhidea contains three species; one is peculiar to Hood, one to Abingdon and Bindloe, and the third is again found in the central islands, Chatham, James, Indefatigable.

Geospiza is represented by eleven species, two of which, *G. dubia* and *nebulosa*, are doubtful. Hood has a peculiar species, *G. conirostis* Rigw. *G. magnirostis* has been found on Charles and Chatham; *G. media*, based on a single specimen, comes from Hood; *G. strenua* has been found on the central and northern islands; *G. fortis*, also *G. fulginosa*, are typical for the central islands; *G. parvula* and *dentirostris* seem to be the original inhabitants of Abingdon and Bindloe.

Cactornis shows five species; *C. brevirostis* is only found on Chatham; *C. assimilis* is typical for Bindloe; *C. abingdonii* for Abingdon; *C scandens* and *pallida* spread over the central islands; no form has so far been found on Hood Island.

Camarhynchus is represented by six species; none is known from Hood; two, *C. prosthemelas* and *C. habeli*, are characteristic of the northern Abingdon and Bindloe; *C. townsendi* has only been found on Charles; the other species occur on the central islands.

As a whole we find in the birds the same distribution as in the reptiles, notwithstanding flight could enable them to reach the different islands. The northern islands, Abingdon and Albemarle, show different forms from the central islands. The same is the case with Hood, so far as it has been examined. Some species of the genus Geospiza, which contains the greatest members, have perhaps only lately spread over a greater number of islands than the others. This genus needs further examination. It seems that in the time when Darwin visited the islands the birds were more

restricted in distribution. I think it possible to trace the "original" locality of each species of this genus.

If we now compare the results we have reached about the distribution and variation of birds and reptiles, we must admit that we have found absolute harmony.

We find exactly the same in regard to the flora, but unfortunately only five islands of the central group have been examined, and that but little,—Chatham, Charles, James, Indefatigable, and Albemarle. All that we know of the flora of Indefatigable, for instance, was collected in a few hours by Andersson. All these islands reach into the humid region. Great results doubtless will be found when the northern and southern islands have been studied, and also the lower ones of the central group, like Barrington and Duncan, for instance. The peculiar genera found in the Galapagos are representcd by peculiar species on the different islands; and the same is true for the non-endemic genera.

(*To be continued.*)

THE BIOLOGICAL WORK OF AMERICAN EXPERIMENT STATIONS.[1]

BY CLARENCE M. WEED.

AS was to be expected, nearly all of the published work of the biological departments of American experiment stations (and for the present purpose I mean, by these, the departments of zoology and botany, including entomology, ornithology, and mycology), up to the present time, has been of a purely practical nature, usually having an immediate application to agricultural procedure. This is perfectly right and exactly what has been needed. There has been, and is yet, a great demand for this sort of work, and it must be satisfied so far as possible if the enterprise is to continue to receive encouragement and support. The problems of spraying, of remedies for the great crop pests, whether insect or fungous in nature, of the effects of rotation and fertilization upon the increase of insects and fungi, and many similar subjects, furnish opportunities for a vast amount of useful work, which must on no account be neglected. But, nevertheless, is there not a tendency to confine ourselves too strictly to these purely practical problems, when very often a somewhat wider study would not only shed light upon the case in hand, but add much useful knowledge besides? Should not our work be organized and conducted on so broad and comprehensive a basis that our results will be cumulative, and in the years to come aid in the formulation of general biological laws?

We are so used to hearing about economic entomology, economic botany, and economic ornithology, that I fear even some of us have an idea that the main field of entomology, or botany, or ornithology, is not economic, but belongs to that department of pure science possessing, in the words recently quoted by Dr.

[1] This paper, in substantially its present form, was prepared for the Entomological Section of the Association of American Agricultural Colleges and Experiment Stations at its recent Champaign meeting, but was not read on account of the limited time at the disposal of the section.

Mendenhall, "the inestimable value of being of no practical utility whatever." But surely this is an error. I believe that every addition to our knowledge of the biology, classification, or distribution of an animal or plant will sooner or later be found useful. The organisms about us are so intimately related to each other and to their environment that we cannot say to one or a few of them: "Thou art of practical account; thee alone will we study." For when we come to investigate even the smallest insect thoroughly, we find that it is simply a part of a vast and complex organism, and that it is vitally related to other creatures in a thousand ways. It is preyed upon by "the fowl of the air and the fish of the sea"; frogs are ever ready to lick it up with their viscid tongues, and toads are lying in wait to send it in search of the mystic jewel within their bodies; quadrupeds in great variety esteem it a delicate morsel, and other insects devour it bodily, or suck out its life-blood, or gnaw away its vitals; bacterial germs are ever ready to destroy it, and the spores of Cordyceps and Empusa are but waiting an opportunity to develop within its body; its alimentary canal may contain a rich and varied fauna and flora, revealed only by the microscope, and the plant upon which it feeds is subject to a thousand agencies that make for its weal or woe. In short, it is engaged in an intense struggle for existence, and an adequate knowledge of the insect itself necessarily involves a consideration of the forces engaged in the struggle.

In the same way, if we would learn the economic status of a bird, we must study its food, its habits, its enemies. To do this properly involves an acquaintance with a large portion of the flora and fauna where the bird occurs,—not a mere knowledge of species, but an acquaintance with their habits and histories. And an adequate knowledge of a plant involves a study of a vast number of organisms,—a study of the insects that attack its roots; those that burrow in its stems; that feed upon its leaves, within or without; that visit its blossoms; that mine its fruit; of those that find in its foliage or flowers concealment from prey or protection from enemies; of the birds that devour its fruit and scatter its seed; of the quadrupeds that browse upon its foliage or gnaw

its bark ; and of numerous other agencies to which it sustains vital relations.

In view of this intimate relation between the organisms with which we have to deal, it seems to me that one of the first needs of our work is the organization of biological surveys of our respective states ; and to this point I invite your special attention.

In that masterly essay published just ten years ago this month, entitled " On Some Interactions of Organisms," Dr. S. A. Forbes stated that " the principal general problem of economical biology is that of the discovery of the laws of oscillation in plants and animals, and of the methods of nature for its prevention and control. " This is as true to-day as it was then, and so also is this paragraph that immediately follows it :

" For all this, evidently, the first, indispensable requisite is a *thorough knowledge of the natural order,—an intelligently conducted natural history survey.* Without the general knowledge which such a survey would give us all our measures must be empirical, temporary, uncertain, and often dangerous. "

At various times in the history of many of the United States provision has been made for more or less complete natural history surveys. As a rule they have been combined with geological surveys, but this has been by no means universal. In Illinois the work has very appropriately been given to the State Laboratory of Natural History, the first of the series of final reports—an elaborate monograph of Illinois birds by Robert Ridgway—having lately been issued. In Kansas such a survey has been undertaken by the scientific staff of Washburn College, and in other western states scientific associations are at work upon it.

It must be confessed that the results of these surveys on the whole are incomplete and unsatisfactory. In no state can there be found a series of volumes containing an adequate account of its flora and fauna. The reason is not far to seek. The provision made for their support has been meagre and insufficient ; to one man has been given the work of ten, and he has often also been compelled to compass in a single season investigations requiring a decade for their proper completion. As a rule the work has also been of a transient nature, and very rarely has it rested on a fairly permanent

basis. A knowledge of the flora and fauna of a state is not to be gained in a day nor a year, nor even, under ordinary circumstances, in a decade. It requires the coöperation of several specialists, with facilities for field and laboratory investigations, rooms for the storage of large numbers of duplicate specimens, a library of American and foreign natural history literature, extensive reference collections, means of producing illustrations, and opportunities for the publication of results,—in short, the methods and equipment of a permanent institution of investigation.

So far as I am aware, Illinois is the only state, unless perhaps we except New York, that has supplied these conditions in a fairly respectable manner. For fifteen years it has maintained a State Laboratory of Natural History, one of the main objects of which has been to make a systematic natural history survey of the state. The work of this laboratory, as published in its series of bulletins, has obtained the most gratifying recognition from the scientific world; and the initial volume of the final reports gives promise of a series of great value and importance. The director is now engaged upon the second volume concerning birds, treating of the food habits and economic relations of the various species, for the Illinois survey is not to be a mere catalogue of forms, but is to include the investigation and discussion of the relations of the organisms to each other and to agriculture.

It is scarcely probable that, for the present, at least, other states will follow to any great extent the example thus set by Illinois. Even there, where the work of Kennicott, Walsh, LeBaron, Riley, Ridgway, Thomas, French, and Forbes has given natural history a prominence and popularity enjoyed by no other western state, it has often been difficult to get the meagre appropriations furnished for the work. But it seems to me that the organization of an experiment station in each state furnishes an opportunity for the conducting of such surveys upon a permanent and well-established basis. Nearly all of them include departments of botany and entomology, with specialists in charge, and in many the college biological departments are closely connected. A knowledge of the flora and fauna of the states would furnish exactly the foundation needed for the prosecution of the distinc-

tively economic work; and the two lines of investigation could very well be carried on together. To illustrate this point let me again quote from a recent report by Professor Forbes, in which the relations of the Illinois State Laboratory of Natural History to the Illinois Station are discussed. He says:

"The recent organization, at the university, of the State Agricultural Experiment Station has raised the question of the relations of the work thus instituted to that of the Natural History Laboratory and the State Entomologist's Office, with the effect to bring about an adjustment of the two at their points of contact in crytogamic botany and economic entomology. The purpose of the State Laboratory being essentially scientific and educational, its results are only incidentally economic; while the purposes of the Experiment Station are essentially economic, and its scientific work must naturally be regulated with close reference to practical results. In cryptogamic botany, for example, the Laboratory is engaged in a general survey of the state, intended to give us the species, the classification, and the life-history of all our flowerless plants, whether economically important or not, and the relations of these to agriculture will come in as a purely secondary matter; while in Experiment Station work, on the other hand, little attention will probably be paid to any species except those having economic relations. *All practical botanists are agreed, however, that the economic species and those of no economic importance are so intimately related in classification, habit, and life-history, that a full and exhaustive knowledge of the whole subject is very helpful, and often indispensable, for the solution of merely economical problems.* The more, in short, the State Laboratory is able to do in technical and biological botany, the easier and more fruitful will be the economic work of the botanical department of the Station. The former should, in fact, supply a broad and strong foundation on which the latter may build elaborately.

"As much of the work in the two directions requires substantially the same facilities, methods, skill, and knowledge, the two may be easily combined in a way to economize labor and expense and to increase results, the only requisite being a common scheme of subdivision and adjustment of proper subjects of research, and

a proper arrangement with respect to assistance, separate and conjoint, in the two departments. Substantially the same may be said of the entomological work."

The Illinois station is fortunate, indeed, to have available the results of the long years of exhaustive work of the laboratory force, but for most of us there is no such reserve fund of knowledge upon which to draw, and if the foundation is obtained we must build it ourselves.

There are other advantages to accrue from such an undertaking besides those to be gained in the direct prosecution of the economic work. One of the greatest of these will be found in the stimulus given to natural history studies. A well-known botanist recently stated that nothing so stimulates the study of a group of plants as a good monograph. In the same way monographs or descriptive catalogues of the organisms of a locality or state are a great help to local naturalists; and there can be no doubt that the existence of volumes similar in plan to that of Ridgway's Illinois birds, treating of the mammals, birds, shells, insects, and plants, would greatly increase the number of students of natural history. We can all remember how eagerly, in our boyhood days, we attempted to get help from books in determining the species, as well as the habits and histories, of the organisms about us. A large proportion of farmers' boys are born naturalists, and it is only because they receive no encouragement and help that so many of them grow away from their early love. It is needless to say that by so increasing the number of nature students the stations would be doing a great service, not only to the agricultural community, but also to themselves,—to the former by adding to the enjoyment of rural life, to the intelligence of the farming community, to the mastery of the farmer's profession; and to the latter by increasing the number of trained observers, to whom experiments and observations may safely be trusted.

Of the methods to be employed in such surveys little need be said at this time. They will necessarily vary with the circumstances and the organizing skill of the individual in charge. But an indispensable requisite in all cases will be that a system of record and arrangement be adopted that is permanent, expansible,

and easily applied, and by means of which the data obtained will be readily available at any time. Much aid also can nearly always be obtained by the judicious use of student assistants and local naturalists, both in making collections and working out results.

I am well aware that this idea will not meet with favor in the eyes of many critics of station work, especially those who are crying for a deluge of immediate practical results, and who often can see nothing "practical" in any result which does not carry with it as a passport the odor of the barnyard, the aroma of the onion bed, or the subtle flavor of insecticides like whale-oil soap. But criticisms from such sources should not prevent the undertaking of the work. The history of all the sciences related to agriculture shows that the investigations of greatest value have been those having to do with the discovery of general laws, and on the surface such investigations have often seemed of the most theoretical and impractical nature.

THE EVOLUTION OF THE CIRCULATORY ORGANS.

BY W. C. CAHALL.

IN the March (1890) number of the AMERICAN NATURALIST I attempted to marshal the evidences furnished by the teeth in support of the hypothesis of evolution. Any other organ or group of organs could have been selected and found equally rich in evidences of a similar import. But none, perhaps, approach quite so nearly to a demonstration, as the beautiful series of cardiac organs met with by the comparative anatomist in his study of zoology.

Origin of Circulatory Organs.—In the lower forms of life, the Protozoa, where no differentiation of structure has yet taken place, the organs of circulation, like those of digestion, are not needed, for every part of the organism performs its own act of digestion and absorption of nutriment. Where a digestive tube is formed, as in the Hydra, the digested food passes by direct absorption into the tissues of the body. In the same group with the Hydra, the Cœlenterata, there are species where from the digestive tube radiate numerous canals which distribute the chyme to every part of the body.

In other species of the same group we see slight but significant and progressive changes in these canals. This "gastro-vascular system," as it has been called, is the first approach to *circulation* we meet with in ascending the animal scale. It is for all practical purposes an efficient circulatory system, yet it is, structurally, nothing more than an amplification of the digestive tract.

The first approach to independence of the organs of circulation from those of digestion is within the Vermes or worms. They have walls and are blood-vessels indeed, since they have a regular circulation of a blood-fluid. A simpler kind is that of some Nemertina, where the main trunks are three long canals connected by transverse shorter ones (Fig. 1).

In Fig. 2 is represented a more complicated system as found

in a higher class of the Vermes, which are furnished with both dorsal and ventral vessels, with pairs connecting them at regular intervals. One or more of these transverse vessels may be enlarged and pulsatile. While at other times the dorsal vessel itself acts as a heart. In this last we are to trace the origin of the heart of both anthropods and vertebrates.

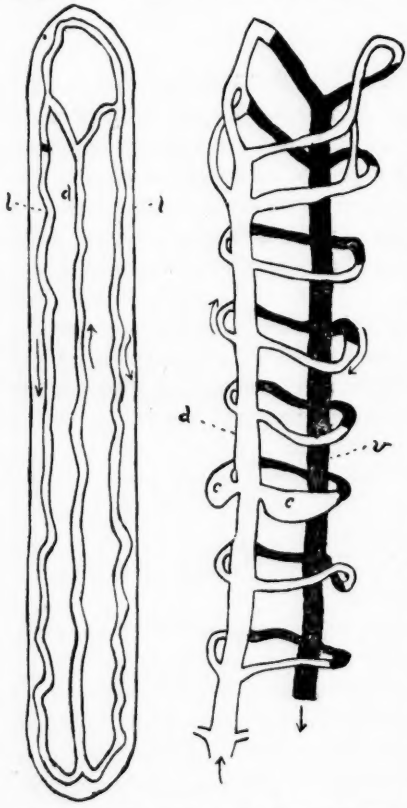

FIG. 1.—Diagram of the vascular system of Nemertina: *d*, dorsal longitudinal trunk; *l*, lateral vessels. The arrows indicate the direction of the stream of blood. After Gegenbaur.

FIG. 2.—Vascular system of *Sœnuris variegata*: *d*, dorsal vessel; *v*, ventral vessel; *c*, heart-like enlargement of a transverse. The arrows indicate the direction of the current of blood. After Gegenbaur.

In the one great group, the Mollusca, there are four types whose several hearts furnish as clear a demonstration of the evolution of an organ as could well be desired. Some reader, unacquainted with comparative anatomy, may even imagine the sketch (Fig. 3) an ideal one by some over-zealous evolutionist, made to confirm his theory; but myriads of these hearts are throbbing to-day as living contradiction to this suspicion. *A* represents a dorsal vessel and transverse trunks of the worm, such as we have already seen in Figs. 1 and 2. *B*. Here we have the single, straight, pulsating ventricles (*v*) with the branching auricles (*a*), as found in the Nautilus. In *C* we have a similar organ of the Loligo, with the auricles

reduced to two. *D* is a diagram of the heart of the Octopus, where for the first time we meet with the organ bent upon itself *E* represents the heart of ventricle and single auricle of the Gastropod, the extreme of development in one direction.

The heart of the fishes likewise consists of two cavities, one auricle and one ventricle, but this is not to be homologised with the two cavities of the molluscan heart. In the Mollusca the auricle receives aërated blood from the respiratory organs, and passes it to the ventricle, which propels the oxygenated blood throughout the body, thus forming a systemic circulation. The Mollusca have no capillaries save in the respiratory organs, so that the blood, after leaving the arteries, flows through canals or lacunæ within the substance of the body. In the fishes, on the contrary, the two cavities convey only venous blood, thus performing the same function as the right side of the heart of mammals. The deoxygenated blood is gathered up from all parts of the body, and conveyed by the veins to the auricle, thence to the ventricle, which organ forces it through the truncus arteriosus into the capillaries of the gills, where he blood is oxygenated by the free oxygen held by the water. The now aërated blood is gathered up by the radices aortæ, and the dorsal aorta disttributes it throughout the body (Fig. 4.) This figure should be compared with Fig. 2, when it will at once be seen, after making allowances for the inverted position of the worm, that the heart of the fish corresponds with the dorsal vessel of that figure, the gill circulation to the transverse vessels of the worm, and the ventral vessel of the one to the dorsal aorte of the other. The resemblances to the Mollusc are largely those of analogy; those to the Annelid, those of true homology.

In the reptiles we see a further development of this central engine of life. Here we have two auricles with one ventricle. The auricle of the fish has had a septum placed down its middle, forming two cavities. In some lower forms this septum is incomplete, but in typical reptiles it is complete. The ventricle also has the rudiments of a septum. Indeed, in some of the higher reptiles, the crocodile for instance, the separation is almost perfect, thus approaching the normal condition of the bird and mammalian

heart. The impure blood which has passed through the system is conveyed to the right auricle, while the left auricle receives the oxygenated blood from the lungs. Thus pure and impure blood become mixed in the ventricle. There are two aortic arches arising from the ventricle, one from the right side and the other from the left. The blood coming through the right aortic arch, now become the pulmonary artery, flows through the pulmonary artery to the lungs, while that entering the left aortic arch is carried throughout the system (Fig. 5).

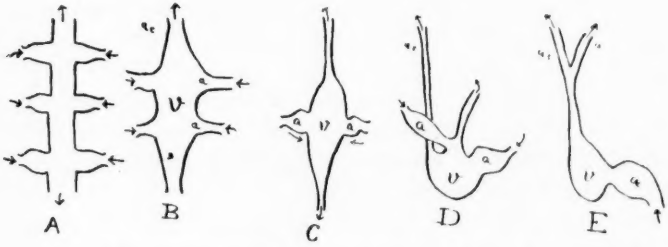

FIG. 3.—CIRCULATORY CENTERS OF MOLLUSCA.—*A*, dorsal and transverse trunks of worm; *B*, heart and auricles of Nautilus; *C*, heart and auricles of Loligo; *D*, heart and auricles of Octopus; *E*, heart and auricles of Gastropod; *v*, ventricle; *a*, auricle; *ac*, arteria cephalica; *a*, arteria abdominalis. The arrows show the direction of the current of blood. After Gegenbaur.

The heart of the bird and of mammals consists of two auricles and two ventricles. The venous blood is gathered up from all parts of the body and emptied into the right auricle, whence it flows through the tricuspid valves into the right ventricle, which by its forcible contraction drives the blood through the pulmonary artery into the lungs. Here the aërated blood is returned to the heart again through the pulmonary veins to the left auricle, thence through the mitral valves to the left ventricle, which sends it bounding throughout the system. There are in the bird and mammal, then, two distinct hearts; the right half, like the heart of the fishes, carrying only venous blood, and the left side, like the heart of the Mollusca, carrying only arterial or aërated blood, while the heart of the reptile is an intermediate organ between the simple apparatus of the fishes and the compound heart of the Mammalia (Fig. VI.).

Thus we have traced, by easy and gradual steps, the complete evolution of the simple pulsating vessel of the Annelid unto the marvelously perfect organ of man. We have seen how the one pulsating tube has divided into two by a partial and then a perfect septum, into an auricle and a ventricle, and then have seen these cavities, by a partition more and more complete, separated into four distinct cavities. Yet all this is done with but slight alterations of preëxisting structure, and without a link in the chain missing This is an argument approaching a demonstration, and must appeal to all candid minds. To those who might object that even the slight changes could not be made without the destruction of the animal or species, I would instance the transformation of every tadpole into a frog. Surely no one will assume that tadpoles are changed now into a frog by any power save that residing in natural laws. Yet the changes are profound.

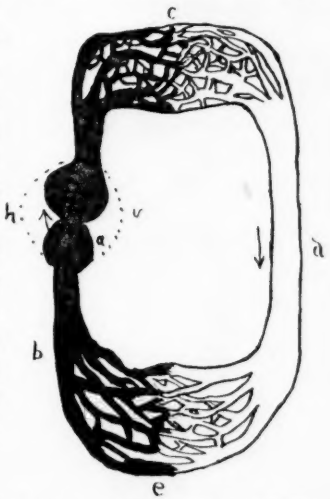

FIG. 4.—CIRCULATION IN FISHES.—*h*, heart, enclosed in pericardium; *a*, the auricle; *v*, the ventricle; *c*, the capillary action in the gills; *d*, the dorsal artery; *e*, the systemic capillaries; *b*, the veins.

The heart of a tadpole is practically that of a fish, having one auricle and one ventricle, and the animal breathes by gills; yet a frog has two auricles and a ventricle, and breathes by lungs. Here we see changes equivalent to the transformation of a fish into a reptile. And among reptiles we meet with hearts with every degree of partition, until in the crocodile the heart is partitioned off very nearly the same as in birds and mammals. In the development of the heart in embryos of birds and mammals we find the organ passing through the conditions found permanently in lower forms. A distinguished comparative anatomist thus outlines the development of the embryo chick: "The first rudiments of the heart appear about the 27th hour, and is a mass of cells, of which the

innermost soon break down, so as to form a tubular cavity; for some time it is simple and undivided, extending, however, through nearly the whole length of the embryo. No motion of fluid is seen in the heart or vessels until the 38th or 40th hour. When the heart, which may be considered analogous at this period to the dorsal vessel of the Annelida, first begins to pulsate, it contains only colorless fluid mixed with a few globules. Between the 40th and 50th hours a separation in its parts may be

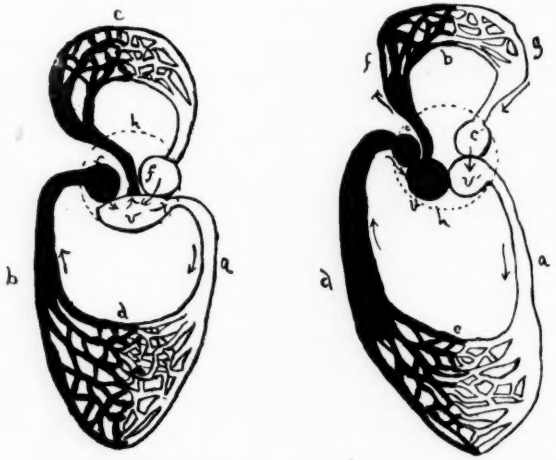

FIG. 5.—CIRCULATION IN REPTILES.—*h*, heart, enclosed in pericardium; *f f*, right and left auricles; *v*, single ventricle; *a*, aorta; *v*, vena cava; *c*, smaller circulation; *d*, greater circulation.

FIG. 6.—CIRCULATION IN MAMMALS AND BIRDS.—*h*, heart; *v*, right ventricle; *v'*, left ventricle; *e*, right auricle; *e'*, left auricle; *a*, aorta; *d*, vena cava; *e*, greater circulation; *f*, pulmonary artery; *g*, pulmonary veins.

observed, which is effected by a constriction round the middle of the tube; and the dilation of the posterior portion becomes an auricular sac, and that of the anterior a ventricular cavity. Between the 50th and 60th hours the tube of the heart becomes more and more bent together until it is doubled, so that this organ becomes much shorter relatively to the dimensions of the body, and is more confined to the portion of the trunk to which it is subsequently restricted. About the same time the texture of the auricle differs

considerably from that of the ventricle; the auricle retaining the thin and membranous walls which it at first possessed, while the ventricle has become stronger and thicker, both its exterior and interior surfaces being marked by the interlacement of muscular fibres, as in the higher Mollusca. About the 65th hour the grade of development of the heart may be regarded as corresponding with that of the fish, the auricle and ventricle being perfectly distinct; but their cavities are as yet quite single. During the fourth day the cavities of the heart begin to be divided, for the separation of the right and left auricles and ventricles. About the 80th hour the commencement of the division of the auricle is indicated, externally, by the appearance of a dark line on the upper part of its wall; and this, after a few hours, is perceived to be due to a contraction, which, increasing downwards across the cavity, divides it into two nearly spherical sacs. The division of the ventricle commences *some time before* that of the auricle, and is effected by a sort of duplicature of its walls. At last, however, the division is complete, and the interventricular septum becomes continuous with the interauricular, so that the heart may be regarded as completely a double organ. The progressive stages presented in the development of this septum are evidently analogous to its permanent conditions in the various species of reptiles. In the heart of mammals (embryo) the same changes take place, but more slowly. Soon after the septum of the ventricles begins to be formed in the interior, a corresponding notch appears on the exterior, which, as it gradually deepens, renders the apex of the heart double.

"This notch between the right and left ventricles continues to become deeper until about the eighth week in the human embryo, when the two ventricles are quite separated from one another, except at their bases; this fact is very interesting from its relation with the similar permanent form presented by the heart of Dugong.

"At this period the internal septum is still imperfect, so that the ventricular cavities communicate with each other, as in the chick, on the fourth day. After the eighth week, however, the septum is complete, so that the cavities are entirely insulated; whilst at the same time their external walls become more connected towards

their base, and the notch between them is diminished; and at the end of the third month the ventricles are very little separated from one another, though the place where the notch previously existed is still strongly marked.

"In the state of the circulatory system in the early embryo, where the heart is as yet but a pulsating enlargement of one of the principal trunks, and the walls of the vessels are far from being complete, we have the representation of its condition in the higher Radiata, and in the lower Articulata and Mollusca. In the subsequent division of the cardiac cavity into an auricle and ventricle an advance is made, corresponding to that which we encounter in passing from the Truncata to the higher Mollusca. And when the branchial arches are formed, which enclose the pharynx and meet in the aorta, the type of the fish is obviously attained, and at a subsequent period the condition of the heart and great vessels presents a strong general resemblance to that of the typical reptiles."

Even at birth the true mammal heart is still incomplete, for there is an opening in the septum between the right and left auricles called the *foramen ovale*, which does not entirely close until after birth, and not in all cases then, leaving the child so formed in a condition almost certain to lead to early death. Does not this opening, which is of no use to fœtus or child, seem more likely to be the result of a general evolution, rather than of a special creation of a useless and oftentimes a harmful accident?

There is also, in the fœtal circulation, a connecting vessel between the pulmonary artery as it emerges from the right ventricle and the aorta as it leaves the left ventricle. This *ductus arteriosus* soon becomes obliterated after birth, so that man has only temporarily what is persistent through life in the reptile.

The peculiar relation of the valves of the veins to the vessels they occupy in man has furnished Dr. S. V. Clevenger, of Chicago, the material for a striking argument for the evolutionary origin of man. (See AMERICAN NATURALIST, Vol. XVIII.)

The veins which return the blood to the heart against gravity, as in the legs and arms, are supplied with valves which allow the

blood to flow upward, yet close and hold the column of blood upon any tendency to regurgitate.

Now this writer claims that the valves of the veins have not yet become accommodated to the upright position of man, for there are several instances in man where the persistence of the valves in certain veins are not only useless for their original purpose, but by their position are actually obstructive to the return of the blood to the heart. He asks, What earthly use has a man for valves in the intercostal veins which carry blood almost horizontally backward to the azygos veins?

When recumbent they are actually a detriment to the free flow of blood. The inferior thyroid veins, which drop their blood into the innominate, are obstructed by valves at their junction. Two pairs of valves are situated in the external jugular and another pair in the internal jugular, but in recognition of this uselessness they do not prevent regurgitation of blood nor liquids from passing upwards. Where apparently most needed, such as venæ cavæ, spinal, iliac, hemorrhoidal, and portal, there are none. The azygos veins have imperfect valves; their rudimentary condition suggests that they may be of recent origin. Now place man on "all fours," and these anomalies disappear. The veins which in man erect do not need valves will be seen to need them against gravity when on "all fours," and as they are found in all four-footed animals; and where, in man erect, those veins which need valves but have them not, when on "all fours" will not need them. Valves in hemorrhoidal veins in quadrupeds would be out of place, yet their absence in man sacrifices many lives and produces untold suffering. It is difficult to escape from the consequences of Dr. Clevenger's logic.

Malformations.—Cyanosis results from the *foramen ovale*, which establishes a communication between the auricles, remaining open after pulmonary respiration had been established, a condition permanent in the crocodile. An arrest of development at an earlier period may cause still greater imperfections in the formation of the heart. Thus, the septum of the ventricles is sometimes found incomplete, the communication between the cavities usually occurring in the part which is last formed, and

which in most reptiles remains open. A still greater degradation in its character has been occasionally evinced, for several cases are now on record in which the heart has preserved but two cavities, an auricle and a ventricle, thus corresponding with that of the fish; and in one of these instances the child has lived for seven days, and its functions had been apparently but little disturbed.

The bifed character of the apex, which presents itself at an early period of the development of the mammalian heart, and is permanent in the Dugong, sometimes occurs as a malformation in the adult subject, evidently resulting, like the others which have been mentioned, from an arrest of development.

The Blood.—The form-elements of the blood itself indicate a parallel evolution with that of the heart and vessels. In the Vermes, where the vascular system is first separated from the digestive tract, the liquid contents known as the blood are generally colorless, occasionally green or reddish in color, and the form-elements are of but slightly different cells. The blood of the Echinoderma (sea-stars and sea-urchins) is of clear or slightly opalescent color, and the form-elements are simple cells.

The blood of the Arthropoda is generally colorless; only in a few insects is it greenish or reddish; even then the color is due to the plasma and not to the cells, which are colorless and of variable size and form, and absent entirely in some of the lower forms, as the Crustacea. The blood of the Mollusca is generally colorless, sometimes bluish, violet, or green; only in one species is the blood red, and then from the plasma, for the blood-cells are simple, undifferentiated, and always colorless.

The blood of crabs and other Crustacea has been proved by M. Fredericq to contain the same saline elements and the same strong and bitter taste as the waters they inhabit. But the blood of sea-fishes is very different. It has not the same constitution as that of the crabs, and shows a marked superiority over them. In fact, the character of the blood-fluid of the invertebrates is strikingly similar to the lymph of the higher vertebrates where the lymphatic and vascular systems are separated. In both, the cells are simple and undifferentiated, colorless, opalescent, or pink.

Even after we enter into an examination of the vertebrates we will meet with a species, and, as we should be led to expect from an evolutionary standpoint, it happens to be of the very lowest class of the vertebrates, the Amphioxus, where the blood-fluid is colorless, and its form-elements are very small, indifferent cells. It is also significant that here also the lymphatic system is not entirely distinct from the vascular system.

But in all other vertebrates, after we leave this lowest class, we shall find the two systems separate, and the blood color red. While the blood is uniformly red, the form-elements of each of the great families of the vertebrates are distinctive and characteristic. The color of the blood now depends upon the coloring matter contained in the blood-cells, and not, as in the few instances of colored blood of the invertebrates, upon the colored plasma. The blood-cells of all vertebrates are highly differentiated, and all contain a nucleus, save the red corpuscles of the highest, the mammals, and even here the nucleus is present in the fœtal stages. The cells are generally flattened. In fishes, Batrachia, reptiles, and birds they are oval and biconvex, while in mammals they are biconcave. The relative quantity of blood in the higher classes of the vertebrates remains the same, yet the relative cell surface varies decidedly.

The red blood-cells are essential to respiration and as carriers of oxygen to the tissues. Fishes consume very little oxygen, and so the red blood-cells are not relatively numerous, and they are called cold-blooded animals, having a temperature but little above that of the surrounding medium. The Batrachia are similarly constituted, but the reptiles have some higher qualites, but still inferior to birds and mammals, which are classed as warm-blooded.

The physiological data contained herein are not the teachings of any special school of science, but the well-digested and generally accepted conclusions of the principal modern authorities on comparative anatomy,—as may be seen more in detail in such works as Carpenter's "Comparative Anatomy," Cope's "Origin of the Fittest," Gegenbaur's "Elements of Comparative Anatomy," Huxley's "Anatomy of Vertebrated Animals," and Owen's "Comparative Anatomy and Physiology."

A FEW NATIVE ORCHIDS.

BY MRS. PRESTON LOVELL.

WHOEVER reads that much-berated production, "The Modern English Novel," remembers the gorgeous young man who disports himself in its pages. However else his attire may vary, in one particular it is invariable: "an orchid in his buttonhole" always adds to it the last touch of elegance. This gorgeous creature may seem a trifle remote from our every-day American civilization, but in this point we may emulate his magnificence. We may, if we will, deck ourselves with the flower which is usually considered beyond the reach of those who cannot build an orchid-house, or seek this latest of fashion's floral favorites in Amazonian forests or the islands of the sea. If you are fearless of bogs and quagmires; if you are ready to tramp through swampy underbrush, disputing territory with snakes and mosquitoes; and if, in addition, you are endowed with what Thoreau named "the instinctive second sight of a flower-hunter," —then let us seek out a few of our wild orchids.

In mid-June, on the low, boggy shores of some lake, we shall find the first-comer, the dainty Arethusa. The flowers of rose purple, borne singly on a short stem, have a curious expectant air, as if a breath of wind would send them fluttering away on their rosy, outspread wings. The closely allied Calopogon differs from the Arethusa in its taller growth and brighter colors. But no orchid is without marked individuality, and we accordingly find the flowers of the Calopogon borne in an apparently inverted position on the stem. It also affords an excellent opportunity to study the strange methods of fertilization peculiar to this order. Few orchids are capable of self-fertilization, depending in most cases upon insect help; and I have often watched the bees coming and going about these flowers, intent only on honey-gathering, but unconsciously working out thereby the fertilization of the Calopogon.

To the sensitive student of plant life every order or family possesses characteristics and peculiarities of its own. I do not here refer to those obvious differences and resemblances upon which classification is based, but something much more intangible, which I do not know how to characterize, otherwise than as difference of *temperament.* In this sense the orchids are a conservative, stay-at-home class, possessed not at all by the spirit of adventure. Other plants may roam far or near in the track of man or beast, but they are impatient of new conditions, and stay firmly rooted in their original haunts. They are a law unto themselves, and usually a law past finding out. Why, for instance, did the quaint Ladies' Tresses (*Spiranthes cernua*) bloom year after year on the edge of the old brick-kiln, and nowhere else by bog or lakeside in the whole vicinity? Indeed, so tenaciously did it cling to this spot, that when years of disuse had dried the kiln I have found the short stems, with their spiral rows of tiny white flowers, among the meadow-grasses, which had usurped the place of the rushes and sedges. And why, of all the lakes scattered throughout the neighborhood, is Clear Lake the only one where the Fringed Purple Orchis (*Platanthera bigelovii*) deigns to rear her splendid spike of rose-red flowers? And this, too, in open defiance of the dictum of the botanist,—"common in wet meadows"! Her sister, the lovely Yellow Fringed Orchis (*P. ciliaris*) does not thus overstep the bounds marked out for her. "Very rare" she is indeed! Only twice have I found the slender stem, crowned by two or three delicate orange flowers, looking like nothing so much as some marvelous insect poised for flight. Once it was the sufficient reward of a long tramp under an August sun to the low-lying meadows which border the Battle Creek; and again, years after, it was the sole trophy of a trip to Hawkin's swamp for huckleberries.

This family trait is also well illustrated by the White Prairie Orchis (*P. leucophæa*). Climax is one of the prairies of small extent scattered throughout Southern Michigan; but small as it is, this characteristic prairie flower has found it out, and blooms there in profusion. Yet a short distance away, under seemingly similar conditions, except the prairie soil, you may search for it

in vain. Just where Climax prairie begins to lose itself in the Jordan marshes you may find the foot-high stem, with its raceme of greenish-white flowers, of the characteristic shape of the Platantheras. In this variety the long, curved, deflexed spur gives to the raceme of flowers a curious, ragged, unkempt appearance.

With the Cypripediums, or Lady's Slippers, few are entirely unfamiliar. "Moccasin flower" the Indians named it, far more appropriately, for its shape is very suggestive of the rounded, soleless moccasin. How vivid is the memory of our childish excursions to Markham's woods! How we searched the dry knolls and oak-crowded uplands for Trilliums, Phlox, Lupines ("Quaker bonnets" we called these), and Painted Cups, but still unsatisfied till we found the Yellow Lady Slipper. This was the supreme reward of our long tramp. In very different environment did we find her dainty cousin, the Pink Lady Slipper. Down in the "bottom-lands," where the sluggish Oonadaga drags through bogs and morasses, where all is shadow and rank growth, there she lifts her delicate cups of pink and white, preaching nature's unending sermon of beauty, purity, and sweetness from filth, decay, and corruption. Rarer than these, but still occasionally to be found by diligent search in swamp or marsh, is the Tall White Lady Slipper.

The time-honored maxim, "All things come round to him who waits," may, for the flower-hunter, be fitly paraphrased, "All things come round to him who *tramps*." For sooner or later, by lonely lake or grassy meadow, on mountain-top or busy side, the flower of his quest will shine before him. So I found the Tiny White Lady Slipper. I had heard of it now and then,—not often, for it is one of the shyest of its shy kind. I had sought for it, in coolness and damp, where it seemed as if it *must* be growing, and once a friend sent me one or two specimens. But at last an early morning walk brought me to the brow of a hill, from whose base a bit of lowland meadow stretched to the banks of Battle Creek. This interval was thickly dotted with the flower of my long search. They stood in patches, in the thick, lush grass, as if a band of fairies had danced the night away on the level greensward, and, fleeing away at my

approach, had left behind their dainty footgear. And dainty indeed must be the feet for such slippers! Into the largest one could scarcely insert the tip of a baby's finger. Pure white, with the gleam of the golden stamens within the tiny sac, the whole plant scarce five inches high, I know of no flower more instinct with mystery and grace.

I have mentioned in this sketch only those orchids with which I am personally familiar. A friend tells me of finding the Rattlesnake Plantain, whose leaves are curiously netted and banded with white, as if its ugly namesake had dragged over them his loathsome length. I think it very probable that this list may be extended, and I am rarely in swamp or marsh that I do not find myself peering curiously around for some strange freak of growth in petal or calyx which shall announce "a new orchid."

RECORD OF AMERICAN ZOOLOGY.

BY J. S. KINGSLEY.

(Continued from Vol. XXIV., page 1169.)

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RECENT LITERATURE.

Sir Samuel Baker on Wild Beasts and Their Ways.[1]—This book will be found very interesting to the general reader, and also instructive to the naturalist. The author restricts his descriptions to what he has actually observed himself, and they therefore have an especial value. As he has hunted in all the continents excepting South America and Australia, his observations cover much ground; in fact, probably no sportsman has lived who can record such a varied experience of wild animals. His observations have also the value which is to be derived from long familiarity with most of the species which he describes. This record is the more useful as many of the species which he has hunted have been already much restricted in numbers and distribution since the author began his career, and some of them are probably doomed to extinction. His accounts of the mental peculiarities and habits of the animals which have come under his observation in a state of nature are very valuable, as such opportunities are rarely enjoyed by persons competent to record them accurately. His book affords, therefore, a mine of information to the student of animal psychology. Sir Samuel Baker is a true sportsman; that is, he observes such methods as will preserve from extinction the species which he pursues, bearing in mind what is remembered too little by the average man with a gun, that if he desires sport in the future he must not destroy females and young, and must protect game sufficiently to ensure its continuance.

His observations cover the larger Mammalia, and include one reptile, the *Crocodilus vulgaris* of Africa. His descriptions of the haunts of these animals will be attractive to all lovers of scenery. They are so exact in detail as to enable the reader to realize it much better than if clothed in more eloquent and enthusiastic language. Incidentally the peculiarities of the people with whom his travels brought him in contact are referred to. Such are the shikaris of India, the hunters of the Hamram Arabs of Abyssinia, and the skin-hunters of the wilds of Western North America.

His description of the habits and manners of the Indian honey-bear (*Melursus labiatus*) are curious. He goes into greatest detail in the history of his experience with the Indian elephant, with whose charac-

[1] Wild Beasts and Their Ways: Reminiscences of Europe, Asia, Africa, and America. By Sir Samuel W. Baker, F.R.S., etc. London, MacMillan & Co., 8vo., pp. 455. Illustrated.

ter he makes his readers fully familiar. We make the following extract, which narrates the behavior of this noble animal when engaged in hunting tigers:

"The foregoing chapter is sufficient to explain the ferocity of the male elephant at certain seasons which periodically affect the nervous system. It would be easy to multiply examples of this cerebral excitement, but such repetitions are unnecessary. The fact remains that the sexes differ materially in character, and that for general purposes the female is preferred in a domesticated state, although the male tusker is far more powerful, and when thoroughly trustworthy is capable of self-defence against attacks, and of energy in work, that would render it superior to the gentler but inferior female.

"It may be inferred that a grand specimen of a male elephant is of rare occurrence. A creature that combines perfection of form with a firm but amiable disposition, and is free from the timidity which unfortunately distinguishes the race, may be quite invaluable to any resident in India. The actual monetary value of an elephant must of necessity be impossible to decide, as it must depend upon the requirements of the purchaser and the depth of his pocket. Elephants differ in price as much as horses, and the princes of India exhibit profuse liberality in paying large sums for animals that approach their standard of perfection.

"The handsomest animal I have ever seen in India belongs to the Rajah of Nandgaon, in the district bordering upon Reipore. I saw this splendid specimen among twenty others at the durbar of the chief commissioner of the central provinces in December, 1887, and it completely eclipsed all others, both in size and perfection of points. The word points is inappropriate when applied to the distinguishing features of an elephant, as anything approaching the angular would be considered a blemish. An Indian elephant, to be perfect, should be nine feet six inches in perpendicular height at the shoulder. The head should be majestic in general character, as large as possible,—especially broad across the forehead, and well rounded. The boss or prominence above the trunk should be solid and decided, mottled with flesh-colored spots; these ought to continue upon the cheeks, and for about three feet down the trunk. This should be immensely massive, and when the elephant stands at ease the trunk ought to touch the ground when the tip is slightly curled. The skin of the face should be soft to the touch, and there must be no indentations or bony hollows, which are generally the sign of age. The ears should be large, the edges free from inequalities or rents, and above all they ought to be smooth, as

though they had been carefully ironed. When an elephant is old the top of the ear curls, and this symptom increases with advancing years. The eyes should be large and clear, the favorite color a bright hazel. The tusks ought to be as thick as possible, free from cracks, gracefully curved very slightly to the right and left, and projecting not less than three feet from the lips. The body should be well rounded, without a sign of any rib. The shoulders must be massive, with projecting muscular development; the back very slightly arched, and not sloping too suddenly towards the tail, which should be set up tolerably high. This ought to be thick and long, the end well furnished with a double fringe of very long, thick hairs, or whalebone-looking bristles. The legs should be short in proportion to the height of the animal, but immensely thick, and the upper portion above the knee ought to exhibit enormous muscle. The knees should be well rounded, and the feet be exactly equal to half the perpendicular height of the elephant when measured upon them whilst standing. The skin generally ought to be soft and pliable, by no means tight or strained, but lying easily upon the limbs and body. An elephant which possesses this physical development should be equal in the points of character that are necessary to a highly trained animal.

"When ordered to kneel, it should obey instantly, and remain patiently upon the ground until permitted to rise from this uneasy posture. In reality the elephant does not actually kneel upon its fore knees, but only upon those of its hind legs, while it pushes its fore legs forward and rests its tusks upon the ground. This is a most unnatural position, and is exceedingly irksome. Some elephants are very impatient, and they will rise suddenly without orders while the ladder is placed against their side for mounting. Upon one occasion a badly trained animal jumped up so suddenly that Lady Baker, who had already mounted, was thrown off on one side, while I, who was just on top of the ladder, was thrown down violently upon the other. A badly tutored elephant is exceedingly dangerous, as such vagaries are upon so large a scale that a fall is serious, especially should the ground be stony.

"A calm and placid nature, free from all timidity, is essential. Elephants are apt to take sudden fright at peculiar sounds and sights. In traveling through a jungle path it is impossible to foretell what animals may be encountered on the route. Some elephants will turn suddenly round and bolt, upon the unexpected crash of a wild animal startled in the forest. The scent, or still worse the roar, of a bear within fifty yards of the road will scare some elephants to an extent that will

PLATE VII.

TIGER DEFYING ELEPHANTS.

make them most difficult of control. The danger may be imagined should an elephant absolutely run away with his rider in a dense forest; if the unfortunate person should be in a howdah, he would probably be swept off and killed by the intervening branches, or torn to shreds by the tangled thorns, many of which are armed with steel-like hooks.

"It is impossible to train all elephants alike, and very few can be rendered thoroughly trustworthy; the character must be born in them if they are to approach perfection.

"Our present perfect example should be quite impassive, and should take no apparent notice of anything, but obey his mahout with the regularity of a machine. No noise should disturb the nerves, no sight terrify, no attack for one moment shake the courage; even the crackling of fire should be unheeded, although the sound of high grass blazing and exploding before the advancing line of fire tries the nerves of elephants more than any other danger.

"An elephant should march with an easy swinging pace at the rate of five miles an hour, or even six miles within that time upon a good flat road. As a rule, the females have an easier pace than the large males. When the order to stop is given, instead of hesitating, the elephant should instantly obey, remaining rigidly still without swinging the head or flapping the ears, which is its annoying and inveterate habit. The well-trained animal should then move backward or forward, either one or several paces, at a sign from the mahout, and then at once become as rigid as a rock.

"Should the elephant be near a tiger, it will generally know the position of the enemy by its keen sense of smell. If the tiger should suddenly charge from some dense covert with the usual short loud but roar, the elephant ought to remain absolutely still to receive the onset, and to permit a steady aim from the person in the howdah. This is a very rare qualification, but most necessary in a good shikar elephant. Some tuskers will attack the tiger, which is nearly as bad a fault as running in the opposite direction; but the generality, even if tolerably steady, will swing suddenly upon one side, and thus interrupt the steadiness of the aim.

"The elephant should never exercise its own will, but ought to wait in all cases for the instructions of the mahout, and then obey immediately.

"Such an animal combining the proportions and the qualities I have described might be worth in India about £1,500 to any Indian Rajah, but there may be some great native sportsmen who would give double that amount for such an example of perfection,—which would combine

the beauty required for a state elephant with the high character of a shikar animal."

The character of the tiger (*Uncia tigris*) is illustrated in following extract:

"I had a practical example of this shortly after the departure of Suchi Khan, when I pushed on to Rohumari and met Mr. G. P. Sanderson, April 1st, 1885. He had brought with him the entire force of elephants from the Garo Hills, the season for capturing wild elephants having just expired. Many of his men were suffering from fever, and he himself evidently had the poison of malaria in his system.

"A bullock had been tied up the preceding evening within three-quarters of a mile from our camp, and on the morning of April 1st this was reported to have been killed. We accordingly sallied out, and in a few minutes we found the remains, above which the vultures were soaring in large numbers. The high grass had been partially burnt, and large patches remained at irregular distances where the fire had not penetrated or where the herbage had been too green to ignite; however, all was as dry as tinder at this season, and having formed the elephants in line, I took up a position with my elephant about three hundred yards ahead.

"The elephants came on in excellent formation, as Mr. Sanderson was himself with them in command. Presently I saw a long tail thrown up from among the yellow grass, and quickly after I distinguished a leopard moving rapidly along in my direction. For a few minutes I lost sight of it, but I felt sure it had not turned to the right or left, and, as a clump of more than ordinary thick grass stood before me, I concluded that the animal had probably sought concealment in such impervious covert.

"When the elephants at length approached, I begged that half a dozen might just march through the patch within a few yards of my position. I was riding an elephant called Rosamund, which was certainly an improvement upon my former mount.

"Hardly had the line entered the patch of grass when, with a short, angry roar, a leopard sprang forward, and passed me at full speed within twenty-five yards, and immediately turned a somersault like a rabbit, with a charge of 16 S. S. G. from the No. 12 fired into its shoulders.

"This was very rapidly accomplished, as our camp was within view, certainly not more than a mile distant.

"We placed the leopard upon a pad elephant, and sent it home; while we once more extended the line, and as usual I took up a posi-

tion some hundred yards in advance, in a spot that was tolerably clear from high grass.

"Almost the same circumstance was repeated. I saw another leopard advancing before the line, and pushing my elephant forward to a point that I considered would intercept it, I distinctly saw it enter a tangled mass of herbage hardly large enough to shelter a calf; there it disappeared from view.

"The line of elephants arrived, and no one was aware that another leopard had been moved. I pointed out the small clump of grass, and ordered an elephant to walk through it. In an instant a leopard bolted, and immediately rolled over like its comrade; but as I had to wait until it cleared the line of elephants before I fired, it was about thirty-five yards distant, and although it fell to the shot, it partially recovered, and limped slowly forward with one broken leg, being terribly wounded in other places. It only went about forty paces, and then lay down to die. One of the mahouts dismounted from his elephant, and struck it with an axe upon the head. The leopard was dispatched to camp, and we proceeded to beat fresh ground, as no tiger had been here, but evidently the two leopards had killed the bullock the preceding night, and nothing more remained.

"Rosamund had stood very steadily, but she was very rough to ride, and the howdah swung about like a boat in a choppy sea.

"A couple of hours were passed in marching through every place that seemed likely to invite a tiger; but we moved nothing but a great number of wild pigs. A few of these I shot for the Garo natives who accompanied us. At length we observed in the distance the waving, green, feathery appearance of tamarisk, and as the sun was intensely hot, we considered that a tiger would assuredly select such cool shade in preference to the glaring yellow of withered grass. At all times during the hot season a dense bed of young tamarisk is a certain find for a tiger, should such an animal exist in the neighborhood. The density of the foliage keeps the ground cool, as the sun's rays never penetrate. The tiger, being a nocturnal animal, dislikes extreme heat; therefore it invariably seeks the densest shade, and is especially fond, during the hottest weather, of lying upon ground that has previously been wet, and is still slightly damp. It is in such places that the tamarisk grows most luxuriantly.

"We were now marching through a long strip of this character, which had at one time formed a channel. On either side the tamarisk strip was enormously high and dense grass. Suddenly an elephant sounded the kettle-drum note. This was quickly followed by several

others, and a rush in the tamarisk frightened the line, as several animals had evidently broken back. We could see nothing but the waving of the bush as the creatures dashed madly past. These were no doubt large pigs; but I felt certain, from the general demeanor of the elephants, that some more important game was not far distant.

"The advance continued slowly and steadily. Presently I saw the tamarisk's feathery tops moving gently about fifteen paces ahead of the line. The elephants again trumpeted, and evinced great excitement. This continued at intervals, until we at length emerged from the tamarisk upon a flat space, where the tall grass had been burned while yet unripe, and, although killed by the fire and rendered transparent, it was a mass of black-and-yellow that would match well with a tiger's color. We now extended the line in more open order,—to occupy the entire space of about two hundred yards front. Sanderson kept this position in the center of the line, while I took my stand in an open space about one hundred and fifty yards in advance, where an animal would of necessity cross should it be driven forward by the heat. The line advanced in good order, but the elephants were much disturbed, for they evidently scented danger.

"They had not marched more than fifty or sixty yards before a tremendous succession of roars scattered them for a few moments, as a large tiger charged along the line, making splendid bounds, and showing his entire length, as he made demonstrations of attack upon several elephants in quick rotation. It was a magnificent sight to see this grand animal, in the fullest strength and vigor, defy the line of advancing monsters, every one of which quailed before the energy of his attack, and the threatening power of his awe-inspiring roars. The sharp crack of two shots from Sanderson, whose elephant was thus challenged by the tiger, hardly interrupted the stirring scene; but as the enemy rushed down the line, receiving the fire from Sanderson's howdah, he did not appear to acknowledge the affront, and having effected his purpose of paralyzing the advance, he suddenly disappeared from view.

"I was in hopes that he would break across the open which I commanded, but there was no sign of movement in the high grass. The line of elephants again advanced slowly and cautiously. Suddenly, at a signal, they halted, and I observed Sanderson, whose elephant was a few yards in advance of the line, halt, and, standing up, take a deliberate aim in the grass in front. He fired. A tremendous roar was the response, and the tiger, bounding forward, appeared as though he would assuredly cross my path. Instead of this, after a rush of

PLATE VIII.

Tragelaphus scriptus.

about fifty or sixty yards, I saw the tall grass only gently moving, as the animal reduced its pace to the usual stealthy walk. The grass ceased moving in a spot within thirty paces, and exactly opposite my position. I marked a bush upon which were a few green shoots that had sprouted since the fire had scorched the grass. I was certain that the tiger had halted exactly beneath that mark. My mahout drove the elephant slowly and carefully forward, and I was standing ready for the expected shot, keeping my eyes well open for an expected charge. Sanderson was closing in upon the same point from his position. Presently, when within a few feet of the green bush, I distinguished a portion of the tiger; but I could not determine whether it was the shoulder or the hind-quarter. Driving the elephant steadily forward, with the rifle to my shoulder, I at length obtained a complete view. The tiger was lying dead!

"Sanderson's last shot had hit it exactly behind the shoulder; but the first right and left had missed when the tiger charged down the line, exemplifying the difficulty of shooting accurately with an elephant moving in high excitement.

"We now loaded an elephant with this grand beast, and started it off to camp, where Lady Baker had already received two leopards. We had done pretty well for the first of April; but after this last shot our luck for the day was ended."

The black African rhinoceros (*Atelodus bicornis*) receives considerable attention, and Sir Samuel's testimony as to its blind ferocity is confirmatory of all that previous authors have told us about it. Of the white rhinoceros (*Atelodus simus*) an interesting account is given. From this we extract the history of the adventure of Oswell, taken from the writings of that African hunter of a previous generation:

"Mr. Oswell was one of the early Nimrods in South Africa, at the same time that the renowned Roualeyn Gordon Cumming was paving the way for fresh adventures. There never was a better sportsman or more active follower of the chase than Oswell. He had gone to Africa for the love of hunting and adventure, at a time when the greater portion was unbroken ground. He was the first to bring Livingstone into notice when he was an unknown missionary, and Oswell and Murray took him with them when they discovered the Lake N'gâmé. He had a favorite double-barreled gun made by Purdy. This was a smooth-bore, No. 10, specially constructed for ball. Although a smooth-bore, it was sighted like a rifle, with back-sights. The gun weighed ten pounds. The owner most kindly lent me this useful weapon when I first went to Africa in 1861, therefore I can

attest its value, and the hard work that it had accomplished. A portion of the walnut stock had been completely worn away to the depth of an inch by the tearing friction of the wait-a-bit thorns, when carrying the gun across the saddle in chase at full speed through the hooked-thorn bushes. The stock had the appearance of having been gnawed by rats.

"At the time of Oswell's visit the country was alive with wild animals, all of which have long since disappeared before the advance of colonial enterprise and the sporting energy of settlers. There was a particular locality that was so infested with rhinoceroses that Oswell had grown tired of killing them, and he passed them unnoticed, unless he met some specimen with an exceptional horn. He was riding a favorite horse, which had been his constant companion in countless shooting incidents, and he happened to remark a large white rhinoceros standing in open ground alone. This animal possessed a horn of unusual length, which made the owner worthy of attention.

"Oswell immediately rode towards it. The animal took no notice of his approach until he arrived within about one hundred yards. The *Rinoceros simus* (white species) is not considered dangerous, therefore he had approached without the slightest caution or hesitation. I forget whether he fired; but I well remember that the beast calmly confronted the horse, and slowly but determinedly, with measured space, advanced directly towards the rider. Like an object in a disturbed dream, this huge creature came on, step by step, leisurely but surely, never hesitating or halting, but with eyes fixed upon the attacking party. Firing at the forehead being useless, Oswell endeavored to move either to the left or right, to obtain a shoulder shot; but the horse, that was accustomed to a hundred contests with wild animals, was suddenly mesmerized and petrified with horror. The quiet and spectre-like advance of the rhinoceros had paralyzed and rooted it to the ground. Trembling all over, its limbs refused to move. The spur and whip were unavailing. The horse felt that it was doomed. This horrible position endured until the rhinoceros was within only a few paces. It then made a dash forward.

"Oswell describes his first sensations, upon returning consciousness, nearly as follows: He found himself upon a horse. The reins were not in his hands. A man was walking in front, leading the animal by the reins, which had been pulled over its head. There were natives upon either side, apparently holding him upon the saddle. A dreamy feeling, and a misty, indistinct view of the situation, was sufficient to assure him that something must have happened. He felt certain that

he must be hurt; but he had no pain. He began to feel himself with his hands, and he felt something wet and soft upon one thigh.

"The fact was, that the long horn of the rhinoceros had passed *through* his thigh. It not only had passed through his thigh, but through the saddle flap, then completely through the horse, and was stopped by the flap upon the other side. The horse and rider together were thrown into the air, and the inversion was so complete that one of Oswell's wounds—a cut upon the head—was occasioned by the stirrup-iron, which proved the inverted position.

"The horse was, of course, killed upon the spot, and the Caffres came to their master's assistance, and placed him upon his spare horse, upon which they held him until they reached the camp. This wound kept the great hunter prostrate for months. It is many years since Oswell told me this story, but I think I have narrated it exactly.

"It must be remembered that this rhinoceros belonged to the so-called harmless species. This incident is sufficient to exhibit the utter fallacy of a belief that any kind of an animal is 'invariably harmless.' We find that many beasts which are accredited with bad characters conduct themselves occasionally as though abject cowards. In the same manner, those which are considered timid may, when least expected, exhibit great ferocity."

The chapter on wild-boar hunting is interesting, and that on the cape buffalo (*Bos caffer*) is especially full of adventure. The habits of the Sambur deer (*Cervus aristotelis*) of India are described with much vividness. Our own hunters will read with interest the adventures of the author in the Big Horn Mountains shooting wapiti (*Cervus canadensis*) and bison (*Bos americanus*).

Altogether we have not had for a long time such a treat as the reading of this book. We give two of the twenty-nine plates with which the book is illustrated.

The Tenth Annual Report of the State Mineralogist of California[1] is a well-illustrated volume, containing a number of general articles descriptive of geological phenomena observed in California during the past year, as well as detailed accounts of the geology of the fifty-three counties into which the state is divided, special reports upon the geology of various mining districts, and upon methods of treating ores. As is to be expected, a large portion of the report is occupied with a discussion of gold mining in its various phases. There is, how-

[1] California State Mining Bureau, William Ireland, Jr., State Mineralogist; Tenth Annual Report of State Mineralogist for year ending December 1, 1890; Sacramento State Office, 1890. Pp. 983, 42 Figs., 7 Pls. and Maps.

ever, in it also much of interest to the general geologist, especially in the essays upon the individual counties, although even in these the greatest emphasis is placed on the geological features of the mines situated within their borders. To the geologist the most valuable portions of the book are the few handsome maps of counties and of mining districts accompanying it, and the mineralogical and geological map of the state, on a scale of twelve inches to the mile. It is proposed in the near future to issue this map by counties on a larger scale. It is unfortunate that California has no geological survey to cooperate with its mineralogical survey in making known to the scientific world the interesting features of its geology hinted at in the report. The state mineralogist feels the need of such a survey, and makes known his desire for it in the opening pages of the volume before us. If a geological survey is instituted, it is to be hoped that its work will be as successful as that of the mineralogical survey.—W. S. B.

Mexicology[1] in our country is a province of archæologic research of but very recent birth. It was inaugurated about 1875 by Raming, and since cultivated, with more or less success, by specialists like Brinton, Bandelier, Thomas, and Valentini. In the person of Dr. Ed. Seler a new ally and collaborating force appears to have joined the ranks of the students enumerated. He comes well prepared for his task. He has traveled extensively in Mexico, and commands the Spanish, Nahuatl, and Maya languages to a high degree. He is in intimate connection with the museums of both America and Europe, and has taken wise care in working only in sight and with the aid of complete literary material,—a luxury which each true scholar longs for, but is rarely able to indulge in. Besides, Dr. Seler possesses that "sense of form" which is so necessary to the true recognition of all the objects drawn, painted, or sculptured, with and which the student of this special branch preëminently has to deal. It was owing to the lack of this artistic sense that some of his predecessors have been lured into the grotesque belief that the ancient Mexi-

[1] Dr. Ed. Seler (Berlin, Kaiser Wilhelm-Strasse, No. 3): (1) "Das Tonalamatl der Aubin' schen Sammlung und die verwandten Kalenderbucher;" 217 pages, with 173 printed illustrations, in Compts Rendus du Congrès International des Américanistes, 7e session, Berlin, 1888. (2) Id., "Alt-mexicanische die Wurfbretter; 12 pages, with 1 colored and 32 printed illustrations; in Internationales Archiv fur Ethnographie, Band III., Berlin. 1890. (3) Id., "Alt-Mexikanische Studien." (*a*) "Ein Kapitel ans dem Geschichtswerk des P. Sahagun." (*b*) "Die sogenannten Sacralen Gefaesse der Zapoteken;" 71 pages, with resp. 108 and 25 printed illustrations; in Veroeffentlichungen aus dem Koeniglichen Museum der Voelkerkunde, I. Band, 4 Heft, Berlin, 1890, bei W. Speeman.

can and Maya records were susceptible of phonetic interpretation. Although, when undertaking to give a full description of Aubin's Tonalamatl (the ritual calendar of the pre-Mexicans), it was not Dr. Seler's object to uproot the aforesaid erroneous theory, yet he did so incidentally. It is a pleasure to see how, under his sagacious guidance, all those curious forms and objects which, influenced by Egyptology, certain students believed to represent letters, syllables, words, and sentences, more or less dissolve, and group together into such objects and paraphernalia as those dress-loving people, men and women, liked to don, to wear, to carry, when going to war or to the temple, or which were in use in their humble households as well as in the sumptuously decorated chambers of their gods and goddesses. We hail the appearance of Dr. Seler's Tonalamatl as a sign and promise of still more work in this direction. Landa's Alphabet at last has become a dead letter. It has not shown from its first publication any trustworthy elements for interpretation, nor had it any claims to be advertised as a new Rosetta Stone.

No. 2, "Alt-mexicanische Wurfbretter" treats of the Mexican "amiento," a sliding apparatus, from which darts and javelins were hurled. This instrument was known to the Eskimos, the Polynesians, and various African tribes, but has been discarded by these peoples, as it had at the time of the Conquest by the Mexicans, according to Dr. Seler's opinion, at least for the purposes of war. In this monograph the author again gives proof of his singular power of identification, finding the picture for the "amiento" in the illustrations embodied in the so-called Mexican Codices, which picture hitherto had been left unrecognized. From thirty diagrams, represented and discussed on the pages of the pamphlet, we learn its various shapes and contrivances, and what is still more interesting, how these various specimens were grasped for action, and held with hand and varying position of the fingers. The correctness of Dr. Seler's recognition is warranted by comparison of the pictured specimens with six real ones recently found in Mexico, and of which three colored illustrations are given. It may here be in place to mention that Christopher Columbus seems to have been the first European to become acquainted with the "amiento," on his fourth voyage on the eastern shores of Chiriqui. He calls it "ballista." From the "amiento," undoubtedly, by the later addition of the bow, the cross-bow has been evolved.

In No. 3 a chapter of a still unpublished work from the pen of Father Sahagua (1570) has been extracted in its original Nahuatl language, with the corresponding Spanish text and illustrations, and an

additional ample discussion of it by Dr. Seler. Fragments only of the padre's great historical work have been known until now, these fragments, however, being so full of valuable, suggestive material to every student of Mexican antiquities that the apparent loss of the whole bulk of the work was universally deplored. It was known from the preserved preface that the padre had taken care to gather from the mouths of competent natives all that was still alive in their memory of the traditional history of their ancestors, of their former social, hierarchic, and political institutions, and that the text of this collective work had been written in the best language of their own, so as to preserve not only the material itself, but this to be also in the clothing of their technical vocabulary and syntactic phraseology. This work has recently been discovered, and in three different copies. One of these is preserved in the Biblioteca Laurentiana of Florence, showing the combined Aztec-Spanish text; the two others in the Biblioteca del Palario and the Biblioteca de la Academia de la Historia, both of them in Madrid, give only the Aztec text. As it appears the printing of the Laurentiana copy has been undertaken at the expense of the Mexican government, it is to be feared that it will be long ere the whole work, embracing twelve volumes, will be in the hands of the students. To quote Dr. Seler's own words: "The publication of Father Sahagua's work would not only be an immense gain to linguistics and Mexican archæology proper, but also to the still unwritten history of the development of this race, of its degree of intellect, and its peculiar notions and conceptions." The paragraphs 5–32, edited and commented by Dr. Seler, are only a "minimal fragment" of the whole, and were selected on account of the richness of the costumes and attributes exhibited in the illustrations of the several deities in discussion.

In an appendix to the previous pamphlet (pages 183–188) a discussion is given on twenty-three Zapotecan figure-vessels, with cuts. In following up the detailed analysis of the characteristic and sumptuous head-dresses that adorn the figure-heads of the aforesaid vessels, we cannot help noticing that what is said of them does not always quite come up to that which we are taught to see. Apart from some splendid identifications which the author's trained eye reveals, and which the student will readily accept, he will miss a comprehensive statement of each of the single components, of their material, their interlacing, their gradual growth, and the final outcome of that enormous "toupée," of which nothing like it is found in the whole ancient and modern history of dress and costume. We are fully aware

it is a problem of no little difficulty for both the "perruquier" and the antiquary. But, on the other hand, if it is to be approached at all, we do not see that its solution will be successfully attained by a refuge to or an introduction of such similies as are taken from Aztec paintings. The two nations differ essentially in their mode of delineation, and still more in that of moulding, carving, and sculpturing. Therefore, in our conception, the true similes for the interpretation of the Zapotecan head-dresses ought to be sought by Dr. Seler in the *cognati* tablets, katanes or steles of Palenque and Copan.—V.

General Notes.

GEOGRAPHY AND TRAVELS.

The Sierra Madre Expedition.—News has been received from the scientific expedition which Dr. Carl Lumholtz is now conducting in the wilds of the Sierra Madre and Northern Mexico.

The expedition started from Bisbee, Arizona, in the early part of September, and, entering Mexico, traveled southward through the State of Sonora, with the intention of crossing the Sierra in the direction of Yanos and Casas Grandes. Before entering the mountain region, however, the explorers separated for a time, and whilst Dr. Lumholtz, with the main body, pursued his intended route, a detachment under Dr. Libbey, of Princeton, made an excursion in a more westerly direction, covering some 300 miles of territory. From Granados the ascent began, and continued steadily until, on December 2d, the western slopes of the Sierra Madre were reached at Nacory, when a northeasterly direction was taken.

Three mountain ranges had to be scaled, the highest some 9,000 feet in height, and the magnificence of the scenery made a strong impression upon the minds of the travelers, who took hundreds of photographs. The weather was very cold. There was snow on the mountain tops, and men and beasts suffered severely in many ways. One man, a guide, whose health was already impaired, succumbed under the strain, and his death was a serious loss to the explorers, as he knew of ruined pueblos to which he had pledged himself to lead them. Several

beasts also perished. After a month of severe exertion the party reached the eastern slope of the Sierra, near Pacheco, and there took a well-earned rest.

The journey had proved a most interesting one from a scientific standpoint. Many specimens of birds and plants were collected, as well as some important fossils.

Cave and cliff dwellings were also met with, some of these in perfect condition and showing signs of having been inhabited by men who had reached a comparatively high stage of culture. In one stairs were found. In the largest of these caves remains of a whole village were discovered, and in front of it stood a huge "olla" (*i. e.*, Mexican water jar), made of clay mixed with straw and very solid, the pottery being eight inches thick. This olla was twelve feet in height and twelve feet in diameter, and when first caught sight of presented the appearance of a huge balloon. In one of the cliff dwellings were found some human remains—a complete skeleton, which had undergone some process of mummification.

The plateau on which the party was encamped when last heard from is near Pacheco, a few days' march from Casas Grandes. The neighboring country is dotted over with many large mounds, some of which it was the intention of Dr. Lumholtz to open. Altogether, the expedition promises well, and there is no doubt that Dr. Lumholtz will bring back much valuable information and make many important additions to our knowledge of the archæology and the natural history of Northern Mexico, past and present.

GEOLOGY AND PALEONTOLOGY.

The Cuyahoga Shales.—C. L. Herrick has published a paper in which he summarizes his studies of the Cuyahoga shale and the Ohio Waverly as follows:

1. The Berea grit is the natural floor of the series, the Bedford shale having its faunal relations decidedly with the shales of the Devonian below.

2. The Bedford forms a striking exemplification of the doctrine of colonies, and that portion lying to the southwest, beyond the western limits of the Erie, retained a fauna derived from the Hamilton long after this fauna had perished to the eastward.

3. The Cuyahoga shales (including the whole series above the Berea so far as present in the Cuyahoga valley) is divisible into three minor sections, the uppermost of which is characterized by a vast abundance of fossils, which are specially well preserved in calcareous or ferruginous concretions, and is a constant and almost unvarying horizon, extending from Lake Erie to the Ohio River. The Cuyahoga proper is never more than 200 feet thick, and forms a transition zone, with a prevailing Devonian habitus.

4. The upper portion of the Waverly is quite distinct from what precedes in fauna, and contains an undoubtedly Lower Carboniferous assemblage.

5. None of the larger divisions of the Carboniferous of the west are entirely unrepresented in Ohio.

6. The transition is nevertheless so gradual that we have an instructive illustration of the evolution of one age from the preceding, with neither catastrophy nor annihilation.

7. There is an opportunity to trace the geological variations in a species as distributed over a great area, and to observe the evolution of new types therefrom.

8. The entire thickness of the Waverly is not far from 700 feet, though the highest consecutive section measures only 670 feet.

9. The Cuyahoga fauna bears an unmistakable resemblance to the so-called Subcarboniferous of Belgium, especially that of Etage I., the limestone of Fornari.

The Pilot Knob of Texas.—Robt. T. Hill has made a study of Pilot Knob in the vicinity of Austin, Texas, and has reached the following conclusions:

"From its structure it is shown that Pilot Knob is the neck of an

ancient volcano which rose out of and deposited its débris in the deep water of the Upper Cretaceous sea (probably Niobrara sub-epoch). From its isolated position, remote from any contemporaneous shore-line, it must have been an island eruption. Pilot Knob probably belongs to a great chain of igneous localities, eruptive and basaltic, extending from the mountains of Northern Mexico to the Ouachita system of Arkansas, both of which regions abound in related features. The great Balcones system of N. 20° E. faults of Central Texas are later than Upper Cretaceous. In late Cretaceous and Tertiary times Pilot Knob was either totally submerged or greatly denuded." (*Am. Geol.*, Nov., 1890.)

The Sierra Nevada of Central California.—During the past season G. F. Becker has studied the structure of the Sierra Nevada Mts. in the neighborhood of the Stanislaus and Truckee Rivers, with the following results:

The whole area in this region has been glaciated up to the summits of passes. There are six systems of fissures. The fissures are fault planes. The disturbances which caused the fissures happened since the close of the Miocene. The faults rarely exceed three inches. A careful study of the vertical fissures leads to the hypothesis of a horizontal thrust acting on a south-southwest to north-northeast line. Mr. Becker advances arguments to show that no important tilting of this portion of the Sierra has taken place at or since the post-Miocene disturbances. The paper closes with the assertion that the theory that the earth is a solid highly viscous mass, is in all respects compatible with the observations, fully explaining every one of the six fissure systems, the faults observed, and the enormous resistance to tilting which the range has displayed. (Bull. Geol. Soc. Am., Vol. II., pp. 49–74).

The Origin of the Great Lakes.—In discussing the origin of the basins of the great lakes of America, J. W. Spencer concludes that the valleys of Lakes Erie, Huron, and Michigan are the result of erosion of the land surfaces by the ancient St. Lawrence River and its tributaries during a long period of continental elevation, and that meteoric agencies had broadened the valleys. This condition was at its maximum just before the Plistocene period. The closing of portions of the old Laurentian valley into water-basins occurred during and at the close of the Plistocene period, owing, in part, to Drift filling some portions of the original valley, but more especially to different warpings of the earth's crust. (*Quart. Geol. Soc.*, Nov., 1890.)

Age of the Glacial Period.—In discussing the cause of the Glacial period, Mr. Warren Upham discards the astronomic theory, since it seems wholly untenable in view of the geologic evidences that not many thousand of years have passed since the departure of the ice-sheets. The measurements of the gorge and Falls of St. Anthony, the surveys of Niagara Falls, the rates of wave-cutting along the sides of Lake Michigan, the rates of filling of kettle-holes, and the rate of deposition in the Connecticut valley at Northampton, Mass., all indicate that the time since the Glacial period cannot exceed 10,000 years. Mr. Upham cites evidence in proof of the theory that the cause of the Glacial period was great uplifts of the glaciated areas, probably in conjunction with important changes in the course and volume of the warm ocean currents. (*Am. Geol.*, December, 1890.)

Geological News.—General.—In a recent paper Mr. E. W. Claypole replies to the four leading arguments for the permanence of the ocean abysses and the continental masses. While he does not advocate the extreme views of Forbes, he gives many good reasons for not adopting the permanence theory in its entirety (Bull. Geol. Soc. Am., Vol. II., p. 10).——Contrary to the general belief that coral reefs are not formed in the western waters of the Gulf of Mexico, Prof. Heilprin and Frank C. Bahn found thirteen species of corals in the neighborhood of Vera Cruz. There are a number of reefs consisting of detached islands extending eastward from the coast nearly six miles. In some cases the greatest development of coral growth is on the lee or shore side. They belong to the same category as the Florida reefs and banks (Proc. Acad. Nat. Sciences, Phila., 1890, p. 303).——In discussing the phosphates of Redonda, a volcanic island in the Caribbean sea, Prof. C. H. Hitchcock maintains that the enormous quantity of mineral precludes the possibility of its having been derived from the droppings of birds, and suggests that it may have come up from below as a phosphuret, which has since changed its character through oxidation and hydration (Bull. Geol. Soc. Am., Vol. II., p. 6).——According to D'Invilliers, the output of guano from the Island of Nevassa is between sixty and seventy-five tons per day. There are two varieties, the gray and the red; the former is the more valuable, since it contains a less percentage of sesquioxide of iron and alumina (Bull. Geol. Soc. Am., Vol. II., pp. 75–84).

Paleozoic.—Mr. A. Smith Woodward considers *Ctenodus interruptus* the Lower Carboniferous representative of the well-known *C. cristatus* (Rept. Yorkshire Philos. Soc., 1889).——According to

H. S. Williams, the Pennine Range of North England affords a typical section upon which the Carboniferous system was founded; and as the term Carboniferous is a misnomer geologically, since coal-bearing rocks are not confined to the system generally so-called, and as the name does not indicate the geographic position of the typical section, he believes that the adoption of the name Pennian System may be of advantage (Bull. Geol. Mag., Vol. II., p. 16).

Mesozoic.—The dentition and dorsal fin-spines of a shark (*Hybodus delabechei*), from the Lower Lias of Lyme Regis, Dorsetshire, have been described by A. Smith Woodward. This specimen is of special interest since it gives the first information as to the number and proportions of the dental series in the jaw of the typical members of the genus to which it belongs (Yorkshire Philos. Soc., 1888).——Some Triassic plants from New Mexico have been described by Wm. H. Fontaine and F. H. Knowlton. They include *Equisetum abiquiense*, *E. knowltonii*, *Zamites powellii* (?), *Z. occidentalis* (?), *Cheirolepis munsterii*, *Palissya braunii* (?), *P.* cone (?), *Cycadites* (?), and *Ctenophyllum* (?). They were found in the shale of a copper mine, and many of the specimens were not well enough preserved to permit of a positive identification. In the sandstone above the shale was found *Araucarioxylon arizonicum* Knowlton (Proc. U. S. Nat. Mus., Vol. XIII., pp. 281–285, Pl. XXII.–XXVI.)——A. Smith Woodward has described a new Pycnodont fish from the English Portlandian bed, and named it *Mesodon damonii*, in memory of one of the most successful explorers of that formation (*Geol. Mag.*, Decade III., Vol. VII., No. 310, p. 158, April, 1890).——A. Smith Woodward announces the discovery of a Jurassic fish fauna in the Hawksbury beds of New South Wales (*Ann. and Mag. Nat. Hist.*, Nov., 1890).——In a recent paper A. Smith Woodward summarizes the skeletal anatomy of the genera Centrolepis and Oxygnathus, and refers two new fishes from the Lower Lias to Cocolepis and Undina respectively, under the names *C. liassicus* and *M. barroviensis* (*Ann. and Mag. Nat. Hist.*, June, 1890).——A. Smith Woodward has recently elucidated some new points in the skeletal anatomy of the genus Eurycormus. This genus has been placed in the same great group as the existing Amia, and the new osteological facts tend to confirm the accepted determination (*Geol. Mag.*, Decade III., Vol. VII., No. 313, p. 289, July, 1890).——According to A. S. Woodward, the fossil fishes of the English oölites are listed as follows: Selachii, 14; Chimæroidei, 11; Ichthyodorulite, 1; Dipnoi, 1; Ganoidei, 18 (Proc. Geol. Ass., Vol. XI., No. 6).

Cenozoic.—A unique siluroid fish from the London clay of Sheppey has been figured and described by A. Smith Woodward. From the character of the fossil its precise affinities cannot be determined, but it closely approaches the living Auchenoglanis of the African rivers. Kŏnig's name of *Bucklandium diluvii* has been retained (Proc. London Zool. Soc., 1889).——Mr. L. C. Hicks has been studying the lagoons of Custer county, Nebraska, and reaches the conclusion that they are the result of sedimentation upon a surface previously shaped by the action of the winds. In other words, the lagoon type is a combination of the sedimentary and æolian types of conformation (Bull. Geol. Soc. Am., Vol. II., p. 25).——In a discussion of the Glacial epoch, F. Leveret presents a line of evidence in support of the theory of two distinct epochs. This evidence is based upon the character of the buried soil and leached till of ten moraines in Illinois, Indiana, and Ohio. The amount of oxidation and leaching would require the lapse of a long interval of time; that is, an epoch of deglaciation in the midst of the Glacial period (Proc. Boston Soc. Nat. Hist., Vol. XXIV., 1889).——According to L. C. Johnston, the flood of muddy waters from the Nita crevasse in the Mississippi River has seriously affected the marine life in the Mississippi Sound. Oyster plantings have been destroyed, and many valuable food fishes have been driven out (Bull. Geol. Soc. Am., Vol. II., p. 20).

ZOOLOGY.

Function of Gemmiform Pedicellariæ of Echinoids.[1]—H. Prouho contributes a very interesting observation to the very vexed question of the functions of pedicellariæ. If a specimen of *Strongylocentrotus lividus* or *Sphærechinus granulatus* be placed in a vessel in which there are one or more specimens of *Asterias glacialis* which have been compelled to fast for some time, the Echinoid will be immediately attacked by the starfishes. As soon as it feels the touch of their ambulacral tubes, it rapidly withdraws its spines from the part threatened; the spines bend out from the center of attack to so great an angle that they become almost tangential to the test. In thus removing its spines the urchin unmasks its gemmiform pedicellariæ, which are then stretched towards the arms of the starfish with the jaws

[1] *Comptes Rendus*, CXI., p. 62, 1890. Abstract from *Jour. Roy. Micros. Socy.*, Oct., 1890, p. 611.

wildly open. The starfish continues its attack, but as soon as one of the pedicellariæ touches an ambulacral tube it immediately bites it; we may suppose that the pain produced is considerable, for the arm of the starfish is actively withdrawn, but it always carries with it the offending pedicellaria fixed in the wound.

In some cases the first bites are sufficient to drive off the starfish, but in others it prolongs the attack, and then it is very interesting to see the urchin unmask its pedicellariæ on the points attacked, and, so to speak, follow the movements of the enemy by showing its teeth. In a first fight the victory is always with the urchin, and the starfish retires covered with wounds. But, as each pedicellaria serves only once for the defense of the urchin, it is gradually deprived of its organs for this purpose. If an urchin is put with several starfishes and abandoned to its fate it succumbs at last.

The moment an Echinoid is warned by its peripheral nervous system of the danger which threatens it, it moves its spines in a way which has nothing in common with the ordinary movements of these organs, and which has no other object than to unmask its gemmiform pedicellariæ. It is of interest to observe that this movement is exactly the opposite of that which is produced when the surface of the test is wounded by, for example, the point of a needle; in that case the spines and pedicellariæ are inclined towards the wounded part.

Hekaterobranchus is the name given by Miss F. Buchanan [2] to a Spionid worm discovered at the mouth of the Thames; but in a postscript she thinks it may belong to Webster's genus Streblospio. The characters are a single pair of dorsal branchiæ situated on the first segment; cephalic tentacles, not grooved but ciliated all over; prostomium well developed; four eyes; first segment prolonged below to form a collar; pharynx evertible and richly ciliated; a single pair of thoracic nephridia, opening on second segment, reaching back to sixth segment, and thence bending forward again.

The Anatomy of Scutigera.—Curt Herbst has discovered some interesting facts regarding this Myriapod. In his Dissertation [3] he describes five systems of glands in the head where he only expected to find the salivary gland described by Dufone. The first is a pair of tubular glands opening at the base of the first maxillæ. The second pair belong principally to the segment of the second maxillæ and open in a deep pit on the side of the head. The third system belongs to the

[2] *Quarterly Jour. Micros. Sci.*, XXXII., p. 175, 1890.

[3] Anatomische Untersuchungen an Scutigera Coleoptrata. Jena, 1890.

same segment and has its openings at the base of the second maxillæ. The fourth and fifth systems are very similar in structure, but differ in the position of their ducts. The fourth opens just behind the second system, the fifth goes through the body wall immediately behind the commissure uniting the dorsal and supraneural vessels. The histology and structure of these systems are detailed. Regarding the functions of these glands Herbst has but little to offer. He thinks that some of them (possibly System III.) may act as spinning glands; while others may play a part in preparing food material. A discussion of the homology of these glands with the head glands of Hexapods and the coxal glands of other Arthropods follows, but our knowledge of these is not sufficient to lead to sure results, though the author considers them as homologous with the coxal glands.

The circulatory apparatus is also described, the most interesting features pointed out being the existence of a cardiac nerve, arising probably from the sympathetic; and the comparison of the supraneural vessel and the arteries on either side of the œsophagus with the similar organs in the Annelids.

The Balancers of Diptera.—Ernst Weinland presents a long and detailed account[4] of his studies of the balancers or halteres in twenty genera of flies. The position, color, hairs, relations, the chitinous skeleton, internal structure, canals, terminal vesicle, nerves and nerve-end structures are described at great length and illustrated by five plates. The results may be summarized in a few words. The balancers are to be regarded as extremely modified wings with internal canals corresponding to those in the "veins" or "nervures" of the true wings. They have not yet lost their powers of motion, a hinge remaining at the base, and in accordance with their position the direction of the flight of the fly is changed. The sense organs with which they are clothed must be regarded as organs of equilibrium.

Nerves of Tortoise Shell.—J. B. Haycraft has noticed the sensitiveness of the carapace of the land tortoise (*Testudo græca*) of Southern Europe. He finds that nerve-fibres penetrate the osseous portion of the carapace and enter a connective-tissue layer immediately beneath the scutes. In this latter they lose their undulated character, and become covered with a dense sheath of tissue. With suitable preparations these nerves are seen to branch, and the ultimate fibres can be traced to the nuclei of the epidermal cells. Not all cells are thus innervated, nor were any nerves found within the shell itself.

[4] *Zeitsch. f. wiss. Zoologie*, LI., p. 55, 1890.

The Cannon-Bone of Ruminants.—The usually accepted view has been that the cannon-bone of the hind leg of the ruminants consists of the coalesced metatarsals three and four, and that the metatarsals two and five become lost during development. J. E. V. Boas now offers evidence[5] which goes far to show that in these forms we are to recognize besides the coalesced metatarsalia three and four the upper ends of metatarsalia two and five. His views are thus in correspondence with those arrived at by various authors in the fossil forms.

EMBRYOLOGY.[1]

Embryology of Limulus.—Professor J. S. Kingsley publishes a preliminary note on the "Ontogeny of Limulus."[2] The segmentation nucleus undergoes several divisions before any signs of segmentation of the egg are seen at the surface. The resulting nuclei migrate towards the surface, and forty hours after impregnation the egg itself begins to cleave, so that the whole becomes separated into cells, with a nucleus in each segment, and a blastoderm forms on one side of the egg. Here the cells are smaller, forming a primitive cumulus, comparable to that of spiders. A circular spot appears in the center of the cumulus, becomes triangular, elongates, and forms a shallow groove,—the blastopore. The mesoderm forms along its margins. Later six pairs of segmentally arranged sensory thickenings appear outside the legs. The first pair gives rise in the median ocelli, the second to a new sense organ, the third disappears, the fourth remains as the "dorsal organ," the fifth gives rise to the paired compound eyes, the sixth is evanescent. All of these organs are connected by a longitudinal nerve. The facts obtained from the ontogeny point to a close relationship between Arachnids and Limulus.

Embryology of Phalangium.—A preliminary note on the early stages of Phalangium is published by Victor Faussek.[3] The egg breaks up into a solid mass of cells, each filled with yolk, and each containing a nucleus. From the large superficial cells there separates by *delamination* small cells, while the resulting small cells form the blastoderm, which soon appears on one side of the egg. The large

[5] *Morph. Jahrbuch.*, XVI., p. 526, 1890.

[1] Edited by Dr. T. H. Morgan, Johns Hopkins University, Baltimore, Md.

[2] *Zoologisches Anzeiger*, No. 345, 1890.

[3] *Zool. Anz.*, January, 1891, No. 353.

yolk-cells in the center of the egg have their nuclei undergo a process of fragmentation, increasing by direct development. The germ-cells (sexual cells) appear when there is but a single-layered blastoderm. A few of the blastoderm cells, which later form the sexual cells, enlarge and form a group of cells which push beneath the surface. The epithelium of the midgut forms from entoderm cells. The nuclei of the yolk-cells form many small nuclei, surrounded by a quantity of plasma lying between the yolk and mesoderm, and soon arrange themselves into the cylindrical epithelium of the midgut. The author points out the correspondence between the early stages of Phalangium and Limulus.

The Embryology of a Scorpion.—Malcolm Laurie publishes a paper under the above title.[4] The earliest stage observed had a small blastoderm at the surface of one end of the egg. This becomes several layered by a process resembling delamination. At a later stage there is a single outer row of cells over one end of the egg, and a thickened mass of cells beneath, some of which are migrating into the yolk. The presence of a primitive groove is doubtful. At the posterior end of the blastoderm there is formed a mass of hypoblast cells, and these may represent invaginated hypoblast. Later a layer of primitive hypoblast cells is to be found under the rest of the blastoderm, and seems to be simply "*split*" from the epiblast. Numerous cells migrate into the yolk. The mesoblast forms under the whole ventral plate from a multiplication of cells of the primitive hypoblast. The origin of the serous membrane and the amnion is described in detail. Cœlomic spaces form in the mesoblast of the segments, and the thoracic appendages contain portions of the cœlom. The coxal glands open at the base of the fifth appendages, and are at first a pair of simple tubes, opening exteriorly at one end and into the cœlom at the other. They seem to be homologous with nephridia. The lateral eyes are as Lankester and Bourne affirmed, monostichous. The central eyes arise by invagination. The stomodæum forms early; the proctodæum much later as a solid plug of cells. The gill-books are appendages comparable to the abdominal appendages of Limulus.

Development of the Fresh-Water Sponge.[5]—Dr. Otto Mass has studied the development from the egg of Spongilla. The first two segments are equal in size and structure, and similarly the 4, 8, and 16 segments, are all alike, giving similar reactions to staining reagents.

[4] *Quart. Jour. Micro. Sci.*, Vol. XXXI., Pt. II.

[5] *Zeit. f. wiss. Zool.*, Band 50, Heft 4.

There results a solid morula. Serial sections demonstrate that at one pole of the morula the cells sink inwards, while the peripheral cells grow over, enclosing a cavity within one end of the egg. Whether this process is a process of growth of cells around one pole of the egg, or whether we have here a process between epibolic and embolic gastrulation, cannot be definitely decided. The cells soon begin to differentiate into tissues, and only the inner ones retain the yolk spherules. The outer layer becomes columnar ciliated ectoderm. Those cells lining the enclosed cavity become flattened at several places, and push out into passages ending in ciliated chambers. Later these latter form the inhalent passages. The remaining cells filling the egg contain yolk, and are the so-called mesoderm cells. Some of these form needles, each needle the product of a single cell, and by their growth push out the ectoderm before them. These changes have taken place while the larva was within the sponge tissues; but it now becomes free and swims about with the pole containing the cavity directed *forwards*. The method of swimming described by Götte, with the pole containing the cavity directed upwards, is undoubtedly pathological. The larval life lasts about twelve hours,—never so long as twenty-four. The best observations on the method of fixation were made with the horizontal microscope. The larva fixes itself by the pole which was directed forwards in swimming,—that is, the end containing the gastric cavity. The cavity itself diminishes. The young sponge flattens to a crust. The high, cylindrical, ectodermal cells become more cubical, then flatten till their longest diameter is tangential to the surface. At first the cilia, one to each cell, were close together, but as the cells flatten they lie farther apart. The above process of fixation and flattening lasts about one-half to three-quarters of an hour. The ectoderm cells around the periphery of the young sponge begin to spread out over the support to which the sponge is fixed, and it takes place by the amœboid-like migrations of the peripheral ectoderm cells. The ectoderm is never thrown off, as Götte supposed, and it seems probable that owing to rough treatment of the embryos they lost their delicate ectoderm. After the fixation of the larva the ciliated chambers—evagination from the inner cavity—come nearer to the surface, fuse with the ectoderm, and form the inhalent orifices. The exhalent orifice originates through a secondary connection of the inner cavity with the outer world.

Descensus Testiculorum.—Under the above title Dr. Hermann Klaatsch, of Heidelberg, has given, in the *Morphologisches Jahrbuch*,

Dec. 16, 1890, what promises to be an important contribution to that most interesting *problema magnum*.

Regarding the recent work upon the actual ontogenetic changes taking place in the human embryo as insufficient for explaining the true morphological descent of the testis in the Mammalia, the author returns to the comparative methods of Hunter and others, and ultimately sees reasons for associating this change of position with changes in other organs,—namely, mammary glands.

From the position of reproductive glands, Wolffian body and body-wall in many mammals the gubernaculum is found to be a complex structure, not entirely homologous in different groups. Thus the testis is first attached to the Wolffian body; only later does the latter become connected with the body-wall by a special "inguinal ligament," which is connected with a peculiar inward process of the muscular body-wall, an "inguinal cone." The separateness of these three parts of a complex gubernaculum is shown in the adult Monotremes.

The phenomena occurring in the periodic descent in the rodents and insectivores furnish the starting point for the interpretation of all other groups.

Here the gubernaculum is chiefly a much-enlarged "inguinal cone," or modified ingrowth of transverse and internal oblique muscles (cremaster). *Pari passu* with the descent of the testis in the adult occurs the evagination of this cone to form a scrotal pouch. This descent appears to correspond to the period of enlargement of the testis; the withdrawal into the body to the period of enlargement of the mammary glands of the female.

In lemurs, apes, and man secondary changes have resulted in the occurrence of the descent once for all in the embryo. Even here the "inguinal cone," though not playing so important a part, in the single descent has muscles resembling those in the rodent. The preformation of a scrotum independent of the descent is not found in all these animals so markedly as in man, where it is to be regarded as a newly acquired falsification of the true record of sequences, and one that is here alone transferred to the female (in the form of the labia majora).

In man, again, an interesting exception to the lack of periodicity occurs,—a reminiscence of a previous adult rodent-like condition being represented by two successive descents in the embryo. Thus a temporary descent has already taken place in embryo of eight cm. This is followed by what is interpreted as a true reditus testium,

subsequent to which the permanent, commonly described descent takes place.

The anatomical relations in the marsupialia, carnivora and ungulates are to be regarded as indicating a separate line of divergence of these groups from the rodent-like conditions. The position of the scrotum is not really so aberrant in the marsupial as to interfere with close comparisons.

A preformed scrotum is here again to be regarded as a falsification of the phylogenetic history, associated with the permanent establishment of the descent in the embryonic period.

From numerous facts, of which I have given an imperfect survey above, the author establishes a connection between the mammary gland and the descent as follows:

The embryo rodent differs from the adult Monotreme in having the gubernacular "inguinal cone."

The descent is not found in the Monotremes and lower vertebrates, and must have arisen in higher mammals.

In these the descent is associated with the modification of the belly-wall, the "inguinal cone." In seeking a cause for the production of this modification of the belly-wall, some external factor and not internal organs is to be considered.

This external factor in the modification of the muscular body-wall was a mammary gland.

The simplest mammary gland is the small area of skin glands with well-developed skin muscles in the inguinal region, on each side, in the Monotremes. This is present in the male also, but is to be considered as secondarily derived from the female.

That such a body might react upon the body-wall is indicated by the existence of the cremaster muscles in the female marsupial, gland and muscle functioning together. Some such change in connection with a mammary gland may have formed the "inguinal cone."

This cone in the male was utilized as being the point of least resistance in the body-wall, and evaginated when the testes enlarged periodically. Though such a cone is present in female animals, it is not associated with the ovary, as this does not enlarge.

The origin of the inguinal ligament remains unexplained, but this also may have been handed over from the female, with other organs connected with the mammary gland.

Granting the scrotum represents such a primary mammary gland area, we would expect to find no true mammary glands in males.

Those present anterior to the inguinal region are easily explained as

recent acquisitions from new organs in the female; while the state of things in the marsupial strengthens the hypothesis. In the lowest Australian marsupial there are no glands in the male, but a scrotal pouch in the place of the female inguinal glands. In Monotremes there are male glands, but no scrotum as yet formed.

The discovery of remnants of such mammary glands in the area into which the testes descend would increase the value of the hypothesis. Such are present in all groups of mammals, conspicuous in apes, and even found in man, in embryos.

The "area scroti" are warty, circumscribed regions of the scrotum, one on each side, in which peculiarities of skin glands, hairs, and especially of skin muscle, form strong contrasts to the rest of the scrotal skin.

These "area scroti" are the externally visible outlines of these primitive mammary organs that gave rise to the descent of the testes.

On the Urinogenital System of the Crocodile and Turtle.[6] —1. There is an undoubted trace of a pronephros in embryos of both, which soon degenerates. 2. A very large glomerulus hanging into the body-cavity on either side. Often the nephrostomes of the pronephros are close to its sides. 3. A segmental arrangement could not be made out for pronephros or glomerulus; no very young embryos were examined. 4. The boundary between pronephros and mesonephros could not be made out, and it was not possible to count the number of nephrostomes belonging to either. 5. The origin of the pronephros—whether from ectoderm or mesoderm—could not be determined. 6. Nephrostomes of the mesonephros often become partially or wholly separated from the body-cavity by a growth upward of the lower lip of the funnel, which surrounds the glomerulus above it. 7. The Müllerian duct is formed entirely independently of the segmental duct, by a folding of the peritoneal ephithelium anteriorly, constricting off the proximal end of the duct, which then grows backwards to the cloaca as a solid rod of cells, which soon acquires a lumen. —J. L. KELLOGG.

The Development of Cyanea arctica.—Since the publication by Louis Agassiz of the third volume of his "Contributions to the Natural History of the United States" no observations have been recorded upon the development of *Cyanea arctica*. During the month of May of the past summer this Medusa was exceedingly abundant in Vineyard Sound and the adjacent waters, and on my arrival at the

[6] R. Weidersheim. *Arch. f. Mik. Anat.*, Band 36, Heft 3, 1890.

Marine Biological Laboratory at Woods Holl, towards the end of that month, I had no difficulty in obtaining large quantities of ova in the earliest stages of development, and I succeeded in keeping the embryos alive until the end of August, by which time they had developed into Scyphistomas with about twenty tentacles.

The developmental history as I observed it differs in so many points from what Agassiz has described, as well as from the observations of other authors upon European forms, that I wish to postpone a detailed account of my observations until I shall have had an opportunity of studying for comparison the embryology of *Aurelia flavidula*, which I hope to accomplish during the coming summer. In the meantime I wish to record here briefly some of the more important facts which I have been able to establish.

The segmentation is practically regular (though the relative size of the first-formed spherules *may*, vary considerably), and results in the formation of a blastula. Certain cells then migrate into the blastocœle, and arrange themselves as an incomplete layer below the cells which remain at the surface, and at the same time an opening appears at one pole of the embryo. This pseudogastrula is, however, very transient. The immigration of cells continues, being apparently multipolar in its distribution, and the opening closes up. Eventually a solid planula or sterrula results, consisting of an external layer of columnar cells and a central mass in which the cell outlines cannot be made out in sections.

In this condition the embryos may persist for some time, swimming about actively. From time to time, however, some settle down to the bottom of the vessel in which they are contained, and enclose themselves in a circular plano-convex cyst. I found a few free-swimming embryos, out of the many hundred which I examined, which had developed a mouth and a central cavity, and possessed a rudiment of a single tentacle, but their further development I was not able to observe. It is certain that the majority encysted themselves in the manner described, but it is of course possible that this may be due to unsatisfactory conditions of life, though the fact that large numbers of the encysted form developed into Scyphistomas argues against such an idea.

While within the cyst, the hollowing out of the central mass and the formation of the endoderm take place. The encysted state lasts for several days, but finally the embryo emerges from the cyst through a circular aperture in the center of the free convex surface of the cyst, formed apparently by solution, as I never saw any ragged edges to the opening. I could not at first believe that the encystment was a stage in the development; it seemed rather to mean the death of the embryos.

The fact that every young Scyphistoma was attached to a cyst, its stalk passing through the opening and spreading out on the lower flat wall, first aroused my suspicions, and I finally succeeded in observing the embryos leaving the cyst, and have sections through forms in various stages of emergence. Encystment has been observed by Kowalewsky in Lucernaria, but was supposed to be a precursor of death. No one has yet observed what I have mentioned above in any Scyphomedusa, but my preparations do not allow of any doubt as to its existence in *C. arctica*.

Shortly after their emergence from the cyst the mouth forms, placing the internal cavity in communication with the exterior, and four tentacles make their appearance. I could not detect any invagination to form the mouth, such as Claus, and especially Goette, have described for other Scyphomedusæ. My preparations show that the ectoderm and endoderm come into contact at the margin of the mouth opening, and that there is no stomatodæal invagination of ectoderm such as Goette maintains exists in Cotylorhiza and Aurelia. It is to be noticed that a similar absence of an ectodermal stomatodæum occurs in *Lipkea ruspoliana*, described by Vogt as the representative of a new tribe of sessile Medusæ, but which, it seems probable, is simply a Scyphistoma.

With regard to the formation of the mesenteries of the Scyphistoma, my results are quite at variance with those of Goette. The young Scyphistomas with four tentacles show no signs of them; in older specimens with the same number of tentacles traces of them are occasionally to be found; but as a rule they are not formed till the young larva has acquired eight tentacles. It is unnecessary to state that in *Cyanea arctica* their formation stands in no connection with the formation of an ectodermal stomatodæum, since this structure does not exist.

An account of the structure of the mesenteries, and the formation of the "trichter" and of the mesenterial filaments, will be given in the complete paper.— J. Playfair McMurrich, *Clark University, Worcester, Mass.*

PHYSIOLOGY.

Vasomotor Nerves of the Portal Vein.—Mall[1] makes an important advance by finding experimental evidence of the existence of vasomotor nerves in the portal vein. This strengthens the idea that this vein and its branches play the rôle of arteries with reference to the capillaries of the liver. If the flow of blood from the aorta to the alimentary canal be stopped, and the splanchnic nerve be stimulated, a narrowing of the portal vein may be detected. If the stimulation be continued, the lumen entirely disappears; at the same time there is an increase in arterial pressure. The subject is to be investigated more fully.

Relation between Molecular Weight, Molecular Structure, and Physiological Action.—Recent work of Gürber[2] on the physiological action of lupetidine and related substances has led Gaule[3] to the conclusion that it is not the weight or size of the molecule that determines the physiological action, but the latter is the product of the effects of the different components of the molecule. If, then, a gradual increase in molecular weight be brought about by the continued addition of a CH_3 group, for example, similarly placed in the molecule, the physiological effects of the compounds so produced will be similar, varying only in degree; but if an NH_2 group be added instead of a CH_3 group, the increase in molecular weight will be essentially the same, but the physiological effects will be different. Thus the physiological effects of drugs will vary with the molecular weight, only when the variation in the latter results from an increase or decrease in the number of identical atomic groups. A second point established by Gürber's work is that different groups of atoms act differently, and that these different groups act on different sets of organs. This would suggest the idea that the living substance in each system of organs represents a peculiar chemical proportion; certain groups of atoms in the body immediately entering into combination with certain groups of atoms in the molecules of substances taken in.—L. G.

Life-History of Blood Corpuscles.—A valuable addition to the literature of the blood corpuscles has appeared recently in the

[1] Archiv für Anatomie und Physiologie. Phys. Abth., Suppl. Bd., 1890.

[2] Archiv f. Anat. u. Phys. Physiol Abthlg., 1890, p. 401.

[3] Ibid., p. 478.

form of a careful paper by Dr. W. H. Howell,[4] of Michigan University. As a result of work extending over a period of two years, most of which was confined to the cat, the author concludes that the corpuscles originate not, as is usually assumed, in different ways, but in accordance with one scheme of reproduction, which is essentially the same in health as in disease, in the embryo as in the adult.

As regards the red corpuscles, these arise in the very young embryo from cords of mesoblastic cells, which outline the position of future veins; the central cells of the cord form corpuscles, while the peripheral ones form the walls of the veins. Such developing blood vessels were found in the liver and in the muscular tissue of the posterior limb, and it seems probable that corpuscles are thus formed wherever there are developing blood vessels. In the second half of embryonic life red corpuscles are formed in the liver, the spleen, and the red marrow. At first this function is most active in the liver, next in the spleen, and lastly in the red marrow. A few weeks after birth, in the cat, the liver and spleen cease to take part in their formation, and in the adult healthy animal they are produced in the red marrow alone. In case of extreme anæmia, resulting from bleeding or whatever cause, the spleen may resume its embryonic function. Wherever and whenever red corpuscles are produced, nucleated forms precede the mature non-nucleated forms, the latter being derived from the former by the *extrusion or migration of the nucleus*,—a process which the author was able to follow in part in the living cell. The life-history of the corpuscle was studied most fully in preparations from the marrow, and is given in brief below. In the very young embryo two forms of red corpuscles occur. One is very large, oval, and always nucleated, which the author regards as possibly an ancestral form. These disappear in early embryonic life. The other, the true mammalian corpuscle, is much smaller, circular in outline, and is found both nucleated and non-nucleated. These apparently arise from colorless, spherical cells —erythroblasts—found in the marrow and elsewhere. The marrow erythroblasts are derived from large embryonic cells, known in the adult simply as marrow-cells,—the unchanged descendants apparently of the original mesoblastic cells from which the marrow is formed. These embryonic marrow-cells multiply by karyokinesis, the daughter cells sooner or later acquiring the structure of the erythroblasts. The erythroblasts multiply rapidly by karyokinesis, giving rise ultimately to cells from which the nucleated red blood corpuscles are derived by the development of hæmoglobin within the cell substance. These

[4] *Journal of Morphology*, Vol. IV., p. 57. 1890.

nucleated red corpuscles multiply also by karyokinesis. When mature they are converted into the ordinary non-nucleated forms by the extrusion of the nucleus. The extruded nuclei are dissolved in the blood plasma, and there is evidence to show that they take part in the formation of fibrinogen. Owing to the loss of the nucleus the corpuscle assumes the biconcave form seen in circulating blood.

The white corpuscles, or leucocytes, arise from the lymphocytes, which are formed in the lymphoid tissue, especially the lymphatic glands. The leucocytes enter the blood apparently as unchanged lymphocytes. Each possesses a single vesicular nucleus, surrounded by a small protoplasmic envelope, and has not the power of making amœboid movements. From this stage the cell develops by growth into a second stage, characterized by a large protoplasmic envelope and amœboid movements. In the third stage the nucleus is drawn out into an elongated strap shape, and may become horseshoe shaped or coiled into a spiral. This cell is actively amœboid, and by the fragmentation of its nucleus becomes converted into the multinucleated leucocyte of the blood. This latter is not, as was formerly thought, a cell in process of division, but rather a disintegrating form, the fragmentation of the nucleus being the first step in the process. The author believes that the fragmented nuclei persist for a time as the blood plates. He is led to this conclusion both by an examination of the leucocytes when in the act of disintegrating, and by the similarity in the appearance and manner of staining of the fragmented nuclei and the blood plates. The disintegrated leucocytes are dissolved in the plasma to form the paraglobulin, which is believed to be derived wholly from this source.

The author discusses fully the work of others and his work has been already reviewed by Minot in the AMERICAN NATURALIST. In addition to the results of actual observation the paper contains a number of interesting suggestions. The most potent of these, perhaps, is the view as to how we may best attack the dark problems concerning the origin and relationship of the blood proteids, and the part which they play in the general metabolism.—E. COOKE.

ENTOMOLOGY.[1]

Dr. Lintner's Sixth Report.—Through the kindness of the author we have been favored with Dr. J. A. Lintner's Sixth Report as State Entomologist of New York. Though less bulky than some of its predecessors, the present volume shows the same painstaking preparation that is characteristic of all of Dr. Lintner's work. The report covers a little more than one hundred pages, illustrated by twenty-five figures, mostly from the writings of Riley, Packard, Glover, etc. After a short introduction of general and popular interest, there is a more or less complete discussion of the following insects: *Eumenes fraternus*, *Hypoderma bovis*, Drosophila sp., *Adalia bipunctata*, *Dermestes lardarius*, *Agrilus ruficollis*, *Coptocycla aurichalcea*, *C. clavata*, *Bruchus scutellaris*, *Hymenorus obscurus*, *Meloë angusticollis*, *Epicauta vittata*, *E. cinerea*, *E. pennsylvanica*, *Pomphopœa sayi*, *Podisus spinosus*, *Prionidus cristatus*, *Pulvinaria innumerabilis*, *Aphis brassicæ*, *Gryllotalpa borealis*, *Melanoplus femur-rubrum*, *Ixodes bovis*, and *Bryobia pratensis* (?). To these accounts a list of publications of the author during 1880, 1881, and 1889 is added as Appendix A, while Appendix B contains a list of contributions to the department.

Sexual Selection in Spiders.—Mr. and Mrs. W. G. Peckham have lately published "Some Additional Observations on Sexual Selection in Spiders of the Family Attidæ,"[2] to which they append an interesting discussion of Mr. Wallace's theory of sexual ornamentation. Observations on the mating habits of an undescribed Habrocestum, *Attus leopardus*, and *Synageles picata* are recorded, showing that the males during courtship so deport themselves that many of the bright markings are displayed before the female to advantage. The authors then take up Mr. Wallace's attempt to explain the superior beauty of male animals without the aid of selection, by attributing it to their greater vigor and activity and higher vitality. "This proposition," the authors state, "is a complexus holding within it three implications which must be proved before its acceptance can be demanded: First, that male animals have higher vitality than females second, that those males that have the highest vitality have also the most brilliant and intense colors; and third, that the superior ornamentation of the males is due to their activity." The authors discuss each of these

[1] Edited by Dr. C. M. Weed, Hanover, N. H.

[2] Occasional Papers of the Natural History Society of Wisconsin, Vol. I., No. 3.

propositions in turn, and, in their concluding summary, state: "We have found that the weak point in Mr. Wallace's argument was in the small amount of evidence that he was able to offer in support of each of the three propositions, so that the successive steps in the argument grew weaker and weaker. Indeed, it seemed to us that although many of his arguments were strikingly ingenious, they all appeared to rest on very slender evidence, or to admit of another interpretation." The mechanical execution of this brochure, like that of its predecessors, is altogether admirable, and several excellent figures by Mr. J. H. Emerton add to the interest of the text.

Oviposition of Dectes spinosus.[3]—Late in the forenoon of the 12th of last July I came upon a female *Dectes spinosus* in the act of depositing an egg in the stem of horseweed (*Ambrosia trifida*). When discovered she had gnawed away the outer fibres of the stem over a small area, and was standing head downward attempting to insert her ovipositor into the stalk. After three trials she succeeded, and the instrument was inserted to its base. About a minute later the posterior portion of the abdomen began to contract and expand, and in less than a minute an egg was placed in the stalk. The beetle then withdrew the ovipositor, and walked rapidly to the top of the plant.

a *b*

The egg was deposited obliquely in the pith on the opposite side of the stem from which the beetle stood. The place of oviposition was about two-thirds of the way from the bottom to the top. The egg is 2 mm. long by 0.3 mm. wide; elongate oval, slightly curved, and of a pale yellow color. It is represented, magnified, at *a* of the accompanying figure, while *b* represents, nearly natural size, a section of the Ambrosia stem with the place of oviposition on its side.—C. M. Weed.

Species of Hymenoptera.—The thirty-seventh fascicle of M. Ed. André's Species des Hymenoptera d'Europe et d'Algerie has lately been issued. It completes the first volume of the Braconidæ, by Rev. T. A. Marshall, and adds about twenty pages to the volume on the Sphegidæ, by M. André, who states that the work on this last-named family is now suspended on account of his inability to use his eyes for microscopic work,—an embarrassment which his entomological brethren will join us in hoping may be speedily terminated.[4] Four excellent

[3] Read before the Entomological Club, A. A. A. S., August, 1890.

[4] Since this was written information has been received of the death of M. André.

colored plates of Braconidæ accompany the fascicle. Future issues are to contain a discussion of the Chrysididæ and Cynipidæ, the former by M. R du Buysson. This admirable series of monographs will prove indispensable to American students of Hymenoptera, and should be in every entomological library.

Papers by Miss Murtfeldt.—The 1889 Report of the Missouri State Horticultural Society contains three excellent papers by Miss Mary E. Murtfeldt. The first, entitled "Outlines of Entomology," contains six chapters discussing the structure, habits, and transformations of insects; the second, "Our Insect Musicians," is a popular discussion of an interesting subject; and the third consists of the Report of the Committee on Entomology for the year. In the last reference is made to the injuries of *Ceresa bubalus, Ceutorrhyncus napi, Lygus pratensis,* and *Gortyna nitela,* each of which did considerable damage in Missouri during the year.

American Tertiary Hemiptera.—Under the title "Physiognomy of the American Tertiary Hemiptera," Mr. S. H. Scudder published a few months ago [5] an important contribution to our knowledge of fossil Hemiptera. It consists of a summary statement of the results of the author's extended study of the subject, with remarks upon the relation of the American Tertiary Hemiptera to those of the present day, and to the Tertiary fauna of Europe. We have room only for the following generalizations: "(1) The general facies of the hemipterous fauna [of North America] is American, and distinctly more southern than its geographical position would indicate. (2) All the species are extinct, and . . . there is scarcely an instance where the same species occurs in two localities. (3) No species are identical with any European Tertiary forms. (4) A very considerable number of genera are extinct, often including numerous species. (5) Existing genera which are represented in the American Tertiaries are mostly American, not infrequently subtropical or tropical American, and where found also in the old world are mostly those which are common to the north temperate zone. A warmer climate than at present is distinctly indicated. (6) There are no extinct families. (7) The appearance of the same familes, and even of the same groups of genera, in the European and the American Tertiaries is common, but of the same restricted genus very rare."

The Polished Harvest Spider.—This handsome species (*Liobunum politum*) was first described in my "Catalogue of the Phalangiinæ

[5] Proceedings Boston Society Natural History, Vol. XXIV., pp. 562–579.

of Illinois,"[6] from three specimens taken about a shed in Champaign county, Illinois. It has not since been discussed.

This harvest spider is an outdoor species, occurring abundantly in fields and woods, although seldom found about barns and outhouses. During the past summer I have taken great numbers in Franklin county, Ohio, in the grass along the banks of a small creek, and among the driftwood left by the overflowing of the Olentangy River. The species becomes fully developed early in July; and the males and females are about equally abundant. Both sexes, when disturbed, emit from the coxal region a liquid having a peculiarly sharp, pungent odor.

I placed a number of these harvest spiders in a large glass vivarium July 10th, 1890. Two days afterward a pair were observed mating. They were standing on one of the vertical sides of the vivarium facing each other. The male kept waving his second pair of legs in the air; his body was somewhat higher than that of his mate, being inclined downward and forward, while that of the latter was inclined upward in front. Similar observations were subsequently made on many other individuals. When alarmed both sexes have a habit of standing on six legs, rapidly vibrating the body, and moving the second legs in a partial transverse circle in the air. In confinement they eagerly devour plant-lice.

The male *L. politum*, is represented, natural size, at Fig. 1, Plate IX. At Fig. 2 are shown the more important structural details, magnified. The body with the legs detached is represented at *a*; *b* represents the eye eminence, side view; *c*, the same, front view; *d*, the palpus, side view; and *e*, the palpal claw.

DESCRIPTION.

Male.—Body, 5 mm. long; 2.8 mm. wide. Palpi, 3.5 mm. long. Legs: I., 25 mm.; II., 51 mm.; III., 26 mm.; IV., 36 mm.

Dorsum smooth, finely granulated; clear reddish-brown, with no markings, except occasionally a faint indication (shown by a slightly darker shade) of the usual central dark marking. Eye eminence rather prominent, slightly constricted at base, black above, canaliculate, with a regular curved series of small, acute, black spines over each eye. Cheliceræ whitish, tips of claws black. Palpi slender, light brown, with femur and patella dusky; finely pubescent, with a subobsolete row of minute dark tubercles on the inner ventro-lateral surface of femur, and another row on the inner ventro-lateral surface of

[6] Bull. Ill. St. Lab. Nat. Hist., Vol. III., pp. 89–90.

PLATE IX.

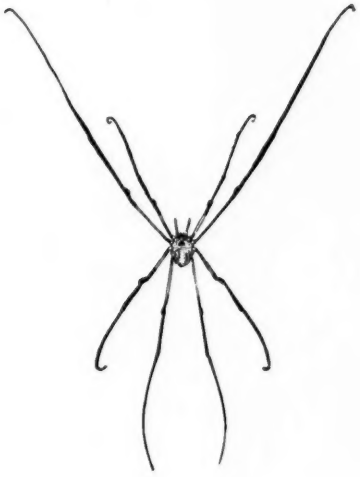

FIG. 1.

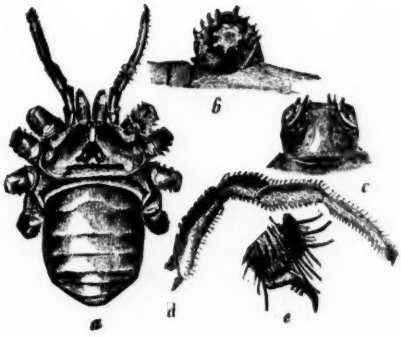

FIG. 2.

Liobunum politum Weed.

tarsus; joints slightly arched. Ventrum with coxæ, including the membranous distal lateral tips, and generally the trochanters, vermilion red. Legs with proximal portions light brown; distally dark brown or blackish. Shaft of genital organ nearly straight, slender, flattened, canaliculate; distal portion very slightly expanded, then slightly contracted, and again expanded into a half spoon-shaped portion, and terminating in a small acute point.

Female.—Body, 6 mm. long; 3.5 mm. wide. Palpi, 4 mm. long. Legs: I., 24 mm.; II., 52 mm.; III., 25 mm.; IV., 38 mm.

Differs from the male in having a larger, rounder body; and in the color of the dorsum, which is brown, with a rather distinct, darker central marking and numerous whitish spots arranged more or less transversely. In some specimens the central marking is subobsolete. Apical rings of ovipositor white. Described from many specimens.

It is a curious fact that while I have found this species one of the commonest harvest spiders in Ohio, especially during 1889 and 1890, I took it but once during three seasons' collecting in Illinois, and have received it but once from outside these two states. Not a single specimen has been found, except in this one case, in the numerous collections received from friends and correspondents in twenty other states. The specimens in my collection represent the following counties in the three states named, the dates given being the time the specimens were collected. All were taken by myself or my assistants, except those from Iowa, which were received from Professor Herbert Osborn. Illinois: Champaign. Iowa: Story (Osborn). Ohio: Champaign, 18 August, 1890; Clermont, August, 1890; Delaware, 18 September, 1890; Franklin, 9 July, 1889, 7, 8, 9, 10, 27, 31 July, 6 August, 2, 5, 6, 9 September, 1890; Lawrence, 5, 6 September, 1890; Madison, 19 July, 1890; Sciota, 3 September, 1890; Warren, 5 July, 14, 16 August, 1890.

It is extremely probable that this species occurs, at least in limited numbers, in most of the central western states.—CLARENCE M. WEED.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

The Biological Society of Washington.—The eleventh anniversary meeting was held in the lecture-room of the Columbian University, on Saturday evening, January 24th, at half-past eight o'clock. The retiring president, Professor Lester F. Ward, delivered an address entitled "Neo-Darwinism and Neo-Lamarckism," in which he took strong ground in favor of the latter doctrine.

February 7th, 1891.—Prof. H. F. Osborn read a paper entitled "A Review of the Cretaceous Mammalian Fauna of North America." It was in effect a review of a paper by Prof. O. C. Marsh upon this subject, in which six new families, sixteen new genera, and twenty-seven new species were described. He illustrated his remarks upon the blackboard, first giving a sketch of the differences between the trituberculate and multituberculate groups of mammals. He then examined in detail the species described as new by Professor Marsh, stating that in no case was more than a single tooth described, and in all cases this was stated to be the upper molar. He stated it as his belief that in numerous instances the teeth were in reality lower molars; and he showed by drawings how teeth referred to distinct species, genera, and even families, seemed to belong to one species. In one instance he mentioned four families, six genera, and seven different species that seemed to belong to one species.

One tooth which had been described as mammalian he thought probably was reptilian, though it was not possible to say positively until the lower part was known.

Professor Marsh was present, and replied to Professor Osborn, stating that he had seen all the specimens of Cretaceous and Jurassic mammals of Europe, and had most of those of Cretaceous age from America in his own collection. He differed *in toto* from Professor Osborn, and had specimens which showed Professor Osborn was entirely mistaken in his assertions. He defended his method of describing and illustrating a single tooth, believing it to be better to describe a single typical example, at least in a preliminary paper, rather than more than one not so perfect or typical. He believed the Cretaceous fauna to have been a large one, the mammals varying from one the size of a shrew to one as large as an opossum; and when he had leisure to describe and illustrate the thousand specimens he now had of Creta-

ceous Mammalia he was convinced the number of species would be increased rather than diminished.

In respect to the tooth considered possibly reptilian by Professor Osborn, he now had a specimen showing it to possess a double fang, so its mammalian character was established. He criticised the use of Multituberculata for a group of mammals, and defended the use of a term applied by himself some years previous to the establishment of the one used instead by the author of the paper.

Dr. Theodore Gill spoke in reference to nomenclature and the value of making priority of proposal of a name the established law in zoology. He believed the proposal of the name, when it was understood to what group or genus or species it was intended to be applied, was of more value than a strict definition. He cited several examples of defective definition; but where the animal to which the term had been applied was well known, he believed it should be recognized.—JOSEPH F. JAMES.

Indiana Academy of Science.—The sixth annual meeting was held at Indianapolis, December 30th and 31st, 1890. Officers and ex officio, Executive Committee of the Academy: T. C. Mendenhall, president; O. P. Hay, John L. Campbell, J. C. Arthur, vice presidents; Amos W. Butler, secretary; O. P. Jenkins, treasurer; D. S. Jordan, J. M. Coulter, J. P. D. John, J. C. Branner, ex-presidents. List of papers read:

Physics and Engineering.—A Set of Resistance Coils and Wheatstone's Bridge, J. P. Naylor; Transformer Tests, A. P. Carman; Note on the Magnetic Permeability of an Impure Nickel at Low Temperature, A. P. Carman; Freezing Process of Excavation, B. A. Lackey; A Brief Description of the New Steam Engineering Laboratory at Purdue University, W. F. M. Goss; A Refraction Rainbow, W. J. Spillman; President's Address—The Work of the U. S. Coast and Geodetic Survey, T. C. Mendenhall.

Chemistry.—Notes on Xylose, W. E. Stone; On Qualitative and Quantitative Reactions for Furfurol, W. E. Stone; On a Pentaglucose Obtained from Corncobs, W. E. Stone and Dumont Lotz; Detection and Estimation of Titanium, W. A. Noyes; A New Method for Quantitative Determination of Albumen in Urine, F. C. VanNuys and R. F. Lyons; An Improved Chemical Test for Blood in Urine, R. F. Lyons; An Apparatus for Determination of Water in Oils and Fats, R. F. Lyons; Oxidation of Phosphoric Acid, H. A. Huston; Albuminoid Nitrogen in Indiana Feeding Material, H. A. Huston.

Geology.—A Recent Find of Musk Ox Remains in Indiana, Joseph Moore; A Review of the Niagara Group in Bartholomew Co., Ind. (by title), J. F. Newsom; Shelby County "Earthquake," J. F. Newsom; Some New Crustacean Fossils, C. E. Newlin; Geological Section at Vincennes, W. J. Spillman; Sections of Drift in Vigo Co., Ind., J. T. Scovell; The Highest Old Shore-line on Mackinac Island, F. B. Taylor; The Effect of the Great Lakes on the Ice Sheet, F. B. Taylor.

Botany.—Preliminary Notes on Genus Polygonum, Stanley Coulter; Aberrant Fruit of *Juglans nigra*, Stanley Coulter; Aberrant Forms of *Juglans nigra*—Structural Changes, D. T. McDougal; Value of Minute Anatomy in Plant Classification, Stanley Coulter; Notes on the Apical Growth of Liverworts, David M. Mottier; Notes on the Germination of Spores of Notothylus (by title), David M. Mottier; A Remarkable Oscillating Movement of Protoplasm in a Mucor, J. C. Arthur; Accelerating Germination by Previous Immersion of the Seed in Hot Water, J. C. Arthur; Notes on Gautemalan Compositæ, Henry E. Seaton; Parasitic Fungi of Indiana, E. M. Fisher; Circulation of Sap, John Morgan; Distribution of Peucedanum in North America, J. N. Rose; Plants Collected by Dr. Palmer in Arizona in 1890, J. N. Rose; Comparative Structure of the Roots of Osmunda and Botrychium, D. H. Campbell; Notes on the Prothallium of the Osmundaceæ, D. H. Campbell; Notes on a New Puccineæ, Henry L. Bolley; On the Manufacture of Plant Infusions for the Culture of Bacteria, Henry L. Bolley; The Occurrence of *Veratrum woodii* in Decatur, Ind., W. P. Shannon; Some Features of the Occurrence of *Viola pedata* var. *bicolor*, Jos. H. Tudor; Preliminary List of Knox County Plants, W. J. Spillman; Introduction of Noxious Weeds, W. J. Spillman; Biological Surveys, John M. Coulter; The Flora of Texas, John M. Coulter; Weight of the Seed in Relation to Production, Katherine E. Golden.

Zoology.—The Identification of Ghost-fishes, Chas. H. Gilbert; The Deep-Water Fishes of the Pacific, Chas. H. Gilbert; The Fishes of the Interior of Kentucky (by title), A. J. Woolman; Notes on Indiana Reptiles, Amos W. Butler; Observations on the Habits of *Synaptomys cooperii*, Amos W. Butler; Chætodontidæ of the Sandwich Islands, O. P. Jenkins; Notes on Structure of Muscle Cells in Salamanders, O. P. Jenkins; Geophila in Jefferson County, Ind. (by title), Geo. C. Hubbard; Notes on Some Actina, W. F. Glick; Some Notes on Indiana Birds, B. W. Evermann; Contribution to the Distribution of the Fishes of the West Coast of North America, O. P. Jenkins and B. W. Evermann; Sailor Spiders on Lake Maxinkuckee, O. P. Jenkins; The Butterflies of Indiana, W. S. Blatchley; The Batrachians and Reptiles

of Vigo Co., Ind., W. S. Blatchley; The Death of Salmon After Spawning, D. S. Jordan; The Fishes of the Upper Columbia and the Shoshone Falls, D. S. Jordan; Eels of America and Europe (by title), D. S. Jordan and B. M. Davis; Food Habits of the Blue Jay, C. W. Hargitt; Notes on *Hydra fusca*, C. W. Hargitt; Acrididæ of Vigo Co., Ind., W. S. Blatchley; On a Bird New to the State Fauna, W. S. Blatchley; On *Cnicus discolor* as an Insect Trap, W. S. Blatchley; Relation of the Number of Vertebræ in Fishes to the Temperature of Water, D. S. Jordan; Notes on Indiana Mammals, B. W. Evermann and A. W. Butler; Audubon's Old Mill at Henderson, Ky., B. W. Evermann; The Range of the Evening Grosbeak in the Winter of 1889–'90, Amos W. Butler; Carolina Parakeet in Indiana, Amos. W. Butler; The Colors of Sounds, Gustaf Kartsen; The Colors of Letters, D. S. Jordan; A List of the Orthoptera of Illinois, with descriptions of new species and observations on the songs and habits of little-known species (by title), Jerome McNeill; Description of a New Æsthesiometer, Wm. Bryan; Researches on the Tactual Perception of Distance, Wm. Bryan; Researches on Reaction Time, Wm. Bryan; Fishes of the Wabash Basin, B. W. Evermann and O. P. Jenkins; Hypnotism, W. B. Clarke.

Addenda.—Notes on Distribution and Habits of *Argynnis diana*, S. G. Evans; Exact and Approximate Formulæ for Calculating the Force at Any Point in the Plane of a Circular Circuit Conveying an Electric Current, Thomas Gray; Some Data as to the Resistance to Cutting of Metals, Thomas Gray; Description of a Powerful Electro-Magnet, with preliminary determination of its magnetic field, C. Leo Mees; Continuation of Experiments in the Change of Density of Metals Under Stress, C. Leo Mees; An Apparatus for Determining Strength of Electric Currents in Absolute Measure, Thomas Gray; Specimens of Diagrams Obtained in Testing Iron and Steel, Thomas Gray; The Relative Magnetic Resistance of Air and Iron, Thomas Gray; On the Solution of the Equation $du = \frac{dx}{(a^2 - x^2)^{\frac{1}{4}}}$ Thomas Gray.

The officers for next year are: President, O. P. Hay; vice presidents, J. L. Campbell, J. C. Arthur, W. A. Noyes; secretary, A. W. Butler; treasurer, O. P. Jenkins.

American Physiological Society.—The annual meeting of the American Physiological Society was held on December 30th, 1890, at the Harvard Medical School, Boston. The following papers were presented: On the Growth of Children, Studied by Galton's Method of Percentage Grades, H. P. Bowditch; A Contribution to Our

Knowledge of the So-called Poisonous Albumens, V. C. Vaughan; Suggestions for the More Effective Advancement of Physiology, Through the American Physiological Society, T. W. Mills; Further Observations in Regard to the Stimulation of Ganglion Cells, C. F. Hodge; On the Vasomotor Nerves of the Heart, H. N. Martin; On Muscle Fatigue, W. P. Lombard; Reaction-time Apparatus, J. G. Curtis.

SCIENTIFIC NEWS.

The last legislature of Alabama made the annual appropriation for the geological survey, $7,500, and continuous,—*i. e.*, till otherwise provided. This places it on a very desirable footing as to permanence, for there will be no effort to bring the survey to a close so long as the state continues to advance in the direction it is now going, and so long as we have anything to report upon. The amount of the appropriation is not so great as could have been wished, but a good deal can be done with it, especially as all engraving, printing, etc., come out of another fund. The survey, under Prof. Eugene Smith, is mapping the Warrior and Coosa coal fields.

Prof. Alpheus Hyatt has published an article in the February *Atlantic Monthly* entitled "The Next Stage in the Development of Public Parks." In this paper Prof. Hyatt advocates the establishment of zoological collections in our public parks, grouped in a way to be of use to the student of zoology. He also suggests the establishment of marine and fresh-water aquaria, and believes that not only fish, but marine animals, insects, water plants, etc., should be shown in them.